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AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,
THE LIBERAL AND FINE ARTS,
AGRICULTURE, MANUFACTURES,
AND COMMERCE.

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Monit. Polit.* lib. i. cap. 1.

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ALERE FLAMMAM.

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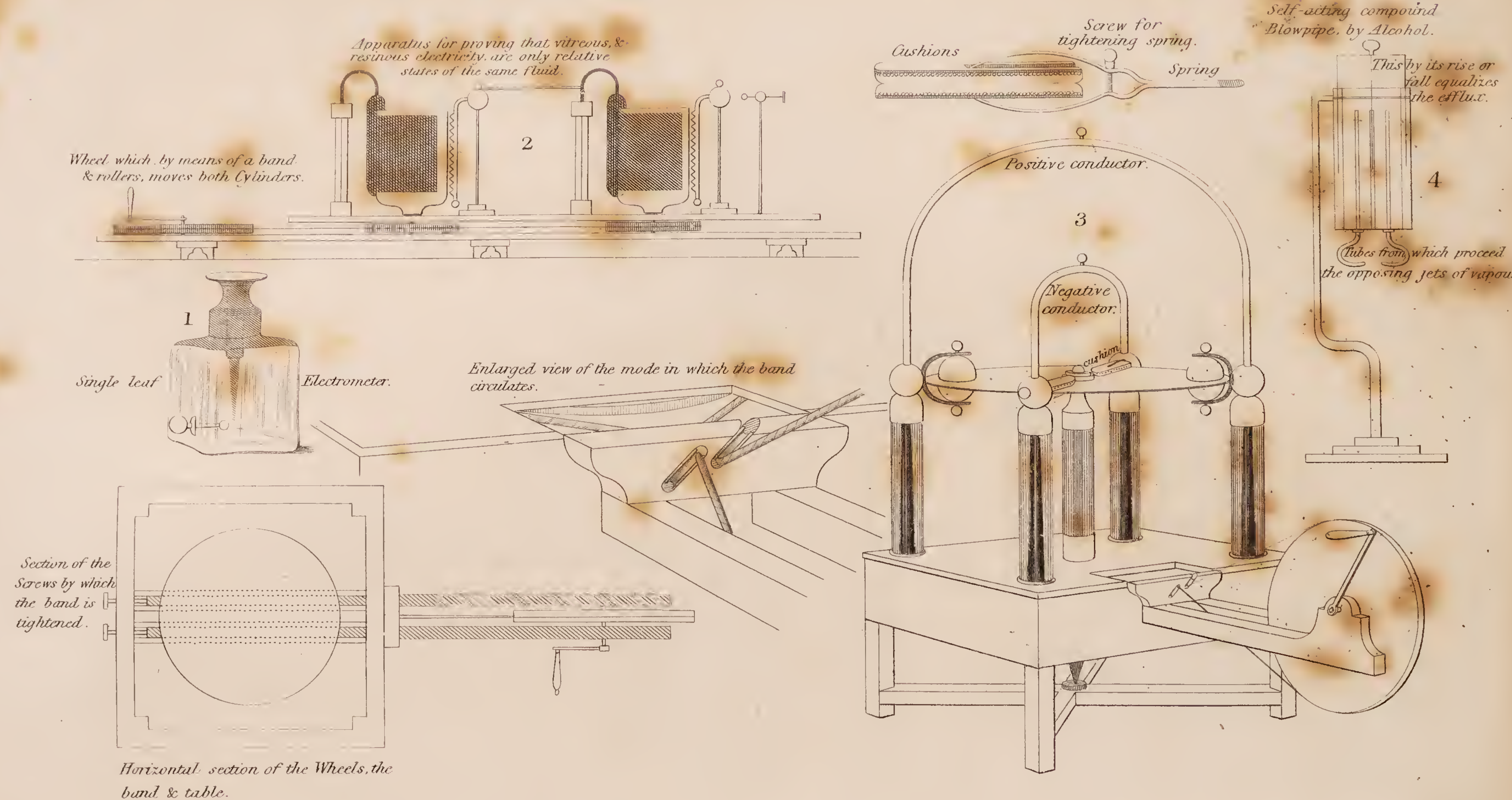

Apparatus for proving that vitreous, & resinous electricity are only relative states of the same fluid.
2
Wheel which by means of a band & rollers, moves both Cylinders.
1
Single leaf
Electrometer.
Enlarged view of the mode in which the band circulates.
Section of the Screws by which the band is tightened.
Horizontal section of the Wheels, the band & table.
Cushions
Screw for tightening spring.
Spring
Positive conductor.
3
Negative conductor.
cushion
Self-acting compound Blowpipe, by Alcohol.
This by its rise or fall equalizes the efflux.
4
Tubes from which proceed the opposing jets of vapour.

S. Porter sc.

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

31st *JULY* 1823.

I. *An Essay on the Question, Whether there be two Electrical Fluids according to* Du Fayè, *or one according to* Franklin. *By* Robert Hare, *M.D. Professor of Chemistry in the University of Pennsylvania**.

BY those who allege the existence of two electrical fluids, much stress has been laid on the fact, that light bodies, when negatively electrified, separate from each other no less than when in the opposite state. The absence and presence of a fluid cannot, it is said, have the same effect of producing repulsion. To this it has been answered, that the separation of such light bodies is not the effect of repulsion, but of an attraction between them and the surrounding medium; which must equally ensue whether they be electrified minus or plus: since in either case that diversity of electrical excitement between them and the surrounding medium arises, which is always productive of attraction.

In support of this view of the question I propose to make a few observations. In an electroscope with moveable coatings, like the galvanometer of Mr. Pepys†, the divergence of the leaves is facilitated in proportion as the coatings are approximated to them, whether the excitement be resinous or vitreous. In this case it must be admitted that there is an attraction between the coatings and the leaves; for, were repulsion between the leaves the cause of their divergence, the approach of the coatings would not increase it.

It may however be supposed, that the repulsion between the similarly excited leaves, being counterbalanced, more or less, in all cases, by the electric tension of the surrounding medium, the coatings may permit the electric fluid to recede through them with greater facility, and thus lessen the electric tension in the direction in which they are situated.

* Communicated by the Author.

† See Phil. Mag., vol. x. p. 38.

Were this supposition to avail in the case of an electrometer with two leaves, it cannot apply in the case of an instrument lately contrived by me, in which, uninfluenced by the idea that repulsion is the cause of electrometrical indications, I suspend only a single leaf. A brass ball, one-fourth of an inch in diameter, is so situated that it may be made to touch the leaf, or retire from it to the distance of an inch, by means of a screw which supports it. (See Plate I. fig. 1.) This instrument is evidently more simple, and is far more sensitive, than any instrument with two leaves heretofore contrived *.

It will be admitted, I presume, that the contact between the ball and the leaf must result from attraction, whether the leaf be minus or plus; and that this would not cease to be true, although a second leaf were, as usual, suspended beside the first.

In a common electrometer, it is usual to have pieces of tin foil pasted on the glass-case opposite the gold leaves. If attraction be exercised between the leaves and coatings, when moveable, it must also be exercised by the fixed coatings thus pasted on the glass. It is therefore established, that when coatings, whether moveable or fixed, are employed, the divergence is not caused by repulsion. It cannot, then, be reasonable to ascribe it to repulsion, though no coatings should be present, as when the leaves are suspended where nothing can attract them unless the surrounding air; especially as the air may be shown competent to perform the same office as the coatings, though not so well, on account of its presenting less matter within the same space. The lightness and mobility of the air is no obstacle to this conclusion. When equally acted upon in all directions, as it must be in the case in point, air resists like an arch, or an elastic solid. The electric attraction may have a tendency to condense it about the sphere of excitement, but cannot move one portion more than another. This opinion of the agency of the air is supported by the fact, that, in proportion as an exhausted receiver is larger, so will the difficulty of producing a divergency in the electrometrical leaves, situated within it, be increased. It would be difficult to procure a receiver so large, that gold leaves might not be made to diverge electrically in it, when exhausted; but leaves of light paper, which will easily be made divergent, in pleno or in vacuo, in a small vessel, will cease to be affected by a like influence, if suspended in an exhausted receiver sufficiently large. I am aware that the air prevents the electric fluid from

* By means of an instrument with a single leaf, since constructed, I am enabled to detect the electricity produced, by one contact between a copper and a zinc disk, each six inches in diameter.

escaping,

escaping, by its insulating power, and that when it is removed, electrometrical leaves cannot be sustained in a state of excitement much higher than the rare medium about them. Thus situated, it may be alleged that repulsion can no more act between them to produce separation, than it does without them to keep them together. But this reasoning would apply equally, whether they be in a large or a small receiver; and, of course, does not account for the influence which the size of the receiver has on the divergency.

I will now adduce some additional facts and arguments, in opposition to the doctrine of two fluids.

According to Franklin, positive and negative, as applied to electricity, merely designate relative states of the same fluid. If, of three bodies, the first have more electricity than the second, and less than the third, it will be positive with respect to the second, and negative with respect to the third. According to Du Faye, there is a radical difference between vitreous and resinous electricity; and though separately exercising intense action, they neutralize each other by union. It is universally admitted, that the fluid evolved by the prime conductor of a glass cylinder machine, and that evolved by the cushion, are of different kinds or states. According to the American theory, the first is positive, the last negative. According to the French theory, the first is vitreous, the last resinous.

Let there be two machines, No. 1 and No. 2, so arranged* that the positive or vitreous conductor of one may communicate with the negative or resinous conductor of the other. In this case, the conductors, thus associated, form effectively but one conducting mass; and one body, with a cushion on one side, and collecting points on the other, might be substituted for both. When this compound apparatus is put into action, it will be found that the intermediate conductor, tested by the resinous conductor of No. 1, is vitreous; but that it is resinous, when tested by the prime or vitreous conductor of No. 2. This result agrees with Franklin's doctrine, as above stated; but how can it be reconciled with the idea that the electricities are radically different, that the same state of excitement may be confounded with either? It may, indeed, be alleged, that the fluid is never completely vitreous, or resinous, or neutral; that although the proportion of either fluid be great, it may still be increased: that one conductor may be more vitreous than a second, but less so than a third—or more resinous than a second, but less so than a third; and hence, in either case,

* See Plate I. fig. 2.

may

may give sparks with either. This is to me, nevertheless, a complicated and unsatisfactory solution of the difficulty.

Pursuant to the Franklinian theory, there can be no really neutral point; though the earth, as a reservoir, infinitely great, compared with any producible by art, furnishes an invariable standard of intensity, above and below which all bodies electrically excited are said to be minus or plus*. It is perfectly consistent with this theory, that sparks should pass, as they are often seen to do, from conductors in either state; not only from one to the other, but to bodies nominally neutralized by their communication with the earth. As the difference between the electrical states of the oppositely electrified bodies, must be greater than between either of their states and that of the greater reservoir, the sparks between them will be longer, but in all other characteristics will be the same. This practical result is irreconcileable with the doctrine of two fluids, according to which there can be no electricity in the earth, which is not in the state of a neutral compound, formed by these opposite electricities. For it would be an anomaly to suppose the re-action between a neutral compound (a *tertium quid*) and either of its ingredients, to resemble in intensity, and in its characteristic phænomena, the re-action which arises between the ingredients themselves. As well might we expect aqueous vapour to explode with hydrogen or oxygen gas, as they do with each other. Nothing can be more at war with the doctrine of definite proportions, of multiple volumes, and every analogy established by the chemistry of ponderable matter, than that two substances should combine, in every possible proportion, and with precisely the same phænomena; that they should be capable of neutralizing each other, and yet eagerly act as if never neutralized.

An argument in favour of the existence of two fluids has been founded on the appearance of two burs, when a card is pierced by an electric discharge. This phænomenon is as difficult of explanation, agreeably to Du Faye's theory, as Franklin's. If a current of electricity, flowing in one direction, should produce a bur, in piercing a card on the side towards which it flows, two currents should be productive of none, one current being precisely adequate to neutralize the

* In some discussions which took place some years ago, between Mr. Donovan and Mr. De Luc, in Nicholson's Journal, it was erroneously charged against Franklin's doctrine, that he supposed that there was an absolute state of neutrality. The doctrine of one universal fluid is to me obviously irreconcileable with that idea, otherwise than as above explained. The quantity of electricity in the globe, is as unalterable in any sensible degree, as the quantity of water in the ocean; and it may therefore be assumed to be invariably the same.

other,

other, according to the premises. The appearance may be explained by either doctrine, as resulting from intense attraction between the paper and the knobs transmitting the discharge.

It has been observed, in favour of the French theory, that, when the hands are made the medium of a feeble discharge, a shock is felt simultaneously in the fingers only of each hand; that, as the shock is made stronger, it affects the wrist, the arm, and finally the chest. This is considered as proving the operation of two distinct fluids; for, were the shock the effect of one current, it would be experienced equally, though feebly, throughout the whole of the circuit. Admitting that such a current were necessary to the discharge, agreeably to Franklin's theory, it ought to be felt most in the fingers, where it is most concentrated, as torrents flow with greater violence in proportion as their channels are narrowed. A current passing from one coating of a Leyden jar to another, is far from being necessary to restore the equilibrium of its surfaces. As soon as a circuit is established between them by the hands, the electricity in the hand which touches the negative surface, flows into it to supply the deficiency; while the hand which touches the positive surface, receives from it a surcharge. It is a case analogous to that of a syphon, in which a fluid, forcibly displaced from the level, is suddenly relieved from restraint; both columns would move at the same time, and with a velocity greater in any part, in proportion as the diameter should be less. The deficit caused in the hand in contact with the negative coating, is supplied by electricity from the arm; and this, again, from the body, where if the charge be inconsiderable, it is so much diffused as not to be perceived. In like manner, a *slight* surcharge received by the hand in contact with the positive coating, is diffused, as it proceeds up the arm to the chest, so as to be too feeble to be felt there.

A piece of tin foil, interposed between paper, has been found not to be perforated by a charge, which had pierced the paper on both sides of it.

If there were but one current, it is alleged that tin foil, situated as above mentioned, would be pierced during its passage from one coating to the other—*a fortiori*, then, it should be pierced, if two currents be necessary, passing each other. Besides, the explanation afforded, in the case of a shock received by the hands, applies to this: owing to its great conducting power, the tin foil diffuses the attraction from each side so much as not to be damaged by it.

II. *De-*

II. *Description of an Electrical Plate Machine, the Plate mounted horizontally, and so as to show both negative and positive Electricity. Illustrated by Engravings. By* Robert Hare, *M.D. Professor of Chemistry in the University of Pennsylvania**.

THE power of electrical plate machines has been generally admitted to be greater than that of machines with cylinders. The objection to the former has been, the difficulty of insulating the cushions, so as to display the negative electricity. Excepting the plate machine contrived by Van Marum, I have read of none in which this difficulty has been surmounted. It is still insisted upon, by respectable electricians, as if it had not been sufficiently removed by his contrivance.

I presume, therefore, that a description of a plate machine, by which both electricities may be shown, and which, after two years' experience, I prefer on every account, may not be unacceptable to the public†.

My plate (thirty-four inches in diameter) is supported upon an upright iron bar, about an inch in diameter, covered by a very stout glass cylinder, four inches and a half in diameter, and sixteen inches in height, open only at the base, through which the bar is introduced, so as to form its axis. The summit of the bar is furnished with a block of wood, turned to fit the cavity formed at the apex of the cylinder, and cemented therein. The external apex of the cylinder is cemented into a brass cap, which carries the plate. The glass cylinder is liable to no strain; it is only pressed where it is interposed between the block of wood within and the brass cap without. The remaining portion of the cylinder bears only its own weight, while it effectually insulates the plate from the iron axis. The brass cap is surmounted by a screw and flange; by means of which, a corresponding nut, and disks of cork, the plate is fastened. A square table serves as a basis for the whole. The iron axis, passing through the cover of the table, is furnished with a wooden wheel of about twenty inches diameter, and terminates below this wheel in a brass step, supported on a cross of wood, which ties the legs of the table diagonally together. The wheel is grooved, and made to revolve by a band, which proceeds from around a vertical wheel outside of the table. This external wheel has two handles: it may of course be turned by means of either of one or both. It is supported on two strips of wood, which by means of screws may be protruded lengthwise from cases, which confine them

* Communicated by the Author.

† See Plate I. Fig. 3.

from

from moving in any other direction. By these means the distance between the wheels may be varied at pleasure, and the tension of the band duly adjusted.

Nearly the same mode of insulation and support which is used for the plate, is used in the case of the conductors. These consist severally of arched tubes of brass, of about an inch and a quarter in diameter, which pass over the plate from one side of it to the other, so as to be at right angles to, and at a due distance from each other. They are terminated by brass balls and caps, which last are cemented on glass cylinders of the same dimensions, nearly, as that which supports the plate. The glass cylinders are suspended upon wooden axes, surmounted by plugs of cork turned accurately to fit the space which they occupy. The cylinders are kept steady below by bosses of wood, which surround them. In this way the conductors are effectually insulated, while the principal strain is borne by the wooden axes.

I consider this mode of mounting an electrical plate preferable to any with which I am acquainted. The friction arising from the band may render the working of the machine a little harder for one person, with one hand; but then it affords the advantage, that two persons may be employed for this purpose, or one may use both hands at once. The intervention of the band secures the plate from being cracked by a hasty effort to put it into motion, when adhering to the cushions, as it does at times; and the screws, by means of which the distance of the wheels is increased, obviate the liability of the band to slacken with wear.

III. *Description of an improved Blowpipe by Alcohol, in which the Inflammation is sustained by opposing Jets of Vapour, without a Lamp: also, of the Means of rendering the Flame of Alcohol competent for the Purpose of Illumination. Illustrated by an Engraving. By* Robert Hare, *M.D. Professor of Chemistry in the University of Pennsylvania.*

In the ordinary construction of the blowpipe by alcohol, the inflammation is kept up by passing a jet of alcoholic steam through the flame of a lamp, supported, as is usual, by a wick—otherwise the inflammation of the vapour does not proceed with sufficient rapidity to prevent the inflamed portion from being carried too far from the orifice of the pipe; and being so much cooled by an admixture of air, as to be extinguished. By using two jets of vapour, in opposition to each other, I find the inflammation may be sustained without a lamp. If one

 part

part of oil of turpentine, with seven of alcohol, be used, the flame becomes very luminous.

In order to equalize and regulate the efflux, I have contrived a boiler, like a gazometer. It consists of two concentric cylinders, opening upwards, leaving an interstice of about one quarter of an inch between them; and a third cylinder, opening downwards, which slides up and down in the interstice. The interstice being filled with boiling water, and alcohol introduced into the innermost cylinder, it soon boils and escapes by the pipes. These pass through stuffing boxes in the bottom of the cylinder. Hence their orifices, and of course the flame, may be made to approach nearer to, or recede further from, the boiler.

The construction of this instrument, which I call the compound blowpipe by alcohol, may be understood from the engraving (Plate I. Fig. 4.).

The idea of making the flame of hydrogen gas, or alcoholic vapour, more luminous by an admixture of oil of turpentine, occurred to me in 1819; and I put the idea into practice in the summer or succeeding winter of that year, when my pupils witnessed the result.

It seems, that Mr. Morey, by another catenation of ideas, was led to a similar inference, employing, in an alcohol blowpipe, whiskey and turpentine. He endeavours so to regulate the efflux of a single jet of the vapour of these fluids, as that it may continue to burn, when once lighted.

This process is too troublesome and precarious for ordinary use. A mixture of alcohol and turpentine are burned with a wick in a lamp, in the same way as oil, according to my plan. It is of course perfectly practicable, and I shall be surprised if it be not adopted in the western country, where alcohol may be had very cheap, and oil must be comparatively dear.

IV. *Remarks on the Trisection of a Circular Arc. By Mr.* PAUL NEWTON.

To the Editors of the Philosophical Magazine and Journal.

Newark, May 4, 1823.

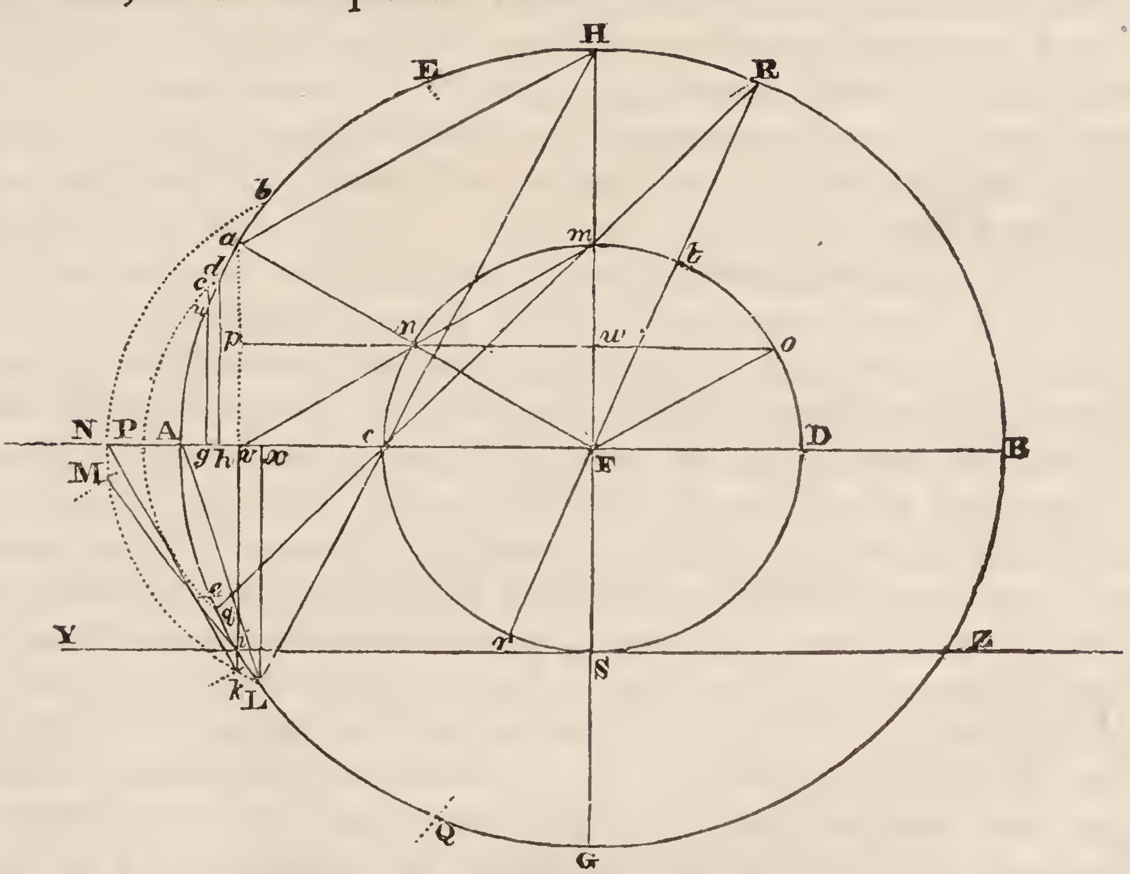
SHOULD the following remarks, the result of much patience, and of many attempts, on the trisection of an angle or a circular arc, obtain a place in your Philosophical Magazine and Journal, they may perhaps prove interesting to some of your mathematical readers. That geniuses so exalted as Newton, Barrow, Halley, in conjunction with an endless list of other illustrious names of our own countrymen, and of celebrated

celebrated foreigners,—that nearly all these authors, ancient and modern, should concur in observing a profound silence on trisection; or that none of them should speak or write thereon, but confidently to pronounce its impossibility, except so far as chiefly relates to a right angle; is discouraging in the extreme, and is sufficient to damp the ardour of the most resolute, zealous, aspiring mind. Hopeless, however, as the performance of this task which I have assigned myself, may still appear to the reader, yet what I have observed permit me to communicate. I am, gentlemen,

Your very obedient servant,

PAUL NEWTON.

With any radius AF, or FB, describe the given circle AEHRB, &c. With half the radius of AF, or of FB, describe the circle *cnmto*D, &c. Let the arc AH, or the arc HB, be that of a quadrant, and let the arc AE be equal to the arc RB, the co-arc EH will in consequence be equal to the co-arc HR. It is required to find a third part of the quadrantal arc AH, or HB, a third part of the arc AE, or RB, a third part of its co-arc EH, or HR, and a third part of the arc AR, or of its equal BE.



To find a third part of the quadrant AH, or HB.—Draw the diameter HG perpendicularly to the diameter AB. Draw the chord H*c*L through the point *c*, in which the circumference of the circle *cnmto*D bisects the radius AF, to meet the given

 circle

circle in L. Draw the sine Lx; the chord LA; and, through s, the extremity of the semi-radius Fs, and in a direction parallel to the diameter AB draw the chord YZ. But the sine Lx, and the semi-radius Fs, being perpendiculars to the parallels AB, YZ, are themselves parallels, and from the nature of parallel lines the sine Lx is longer than Fs. And because the third part of a quadrant $=30$ degrees, and the sine of 30 degrees = half the radius, the sine Lx, being longer than the half radius Fs, exceeds its proper magnitude. The sine Lx is here considered as = the sine of $\frac{1}{3}$d part of the quadrantal arc AH or HB augmented by an arc commencing at L, and described from the centre c, with the radius Lc, part of the chord LcH. With cL, therefore, as radius, and centre c, describe the arc LkMNb, forming a lune with the given circle, and meeting or intersecting the given circle in the points L, and b, at equal distances from N, the extension of the diameter AB. With the chord AL as a distance, and L as a centre, describe an arc at M. With the same distance AL, and centre N, describe an arc at k. The arc NM = the arc kL. From which I infer that if we apply the chord AL, from N to k, on the lunar arc LkMNb, the point k fixes the situation of the sine kiv, drawn of course perpendicularly from the point k, or parallel to the sine Lx; and the point i, in which the sine kiv intersects the given circle, is one extremity of the true sine; the other extremity v is consequently on the diameter; and iv = Fs is = the sine of $\frac{1}{3}$d part of the quadrant AH, or HB.

For the sake of illustration, let us suppose the radius cL, the sine Lx, and the chord LA, to be composed of, or represented by, three inflexible rods. Let the extremity c of the radius Lc be a fixed point, and let the chord LA, the sine Lx, and the radius Lc, all meeting in the point L, be so joined or attached to one another in this point, that the end A of the chord or rod LA shall, in quitting the arc LA, fall upon the lunar arc LkMNb at M, and that while the extremity L of the lunar radius cL, bearing with it the sine Lx, shall move along the arc LkMNb, from L to k; the extremity M of the chord or rod LM shall be driven along the same arc LkMNb, from M to N, and shall lie in the direction kN, during which time and motion the sine Lx, preserving its perpendicular direction, is carried into the situation of the sine kiv; for the arc MN is = the arc kL. It hence appears that the chord LA, applied to the arc LkMNb, leaves a remainder of this arc MN, or kL, = the intervening arc which separates the sine Lx from the sine kiv; the part iv of the latter of which

is

is the true sine. For, produce now the sine iv, to meet the given arc in a on the opposite side of the diameter AB. Then because the chord of an arc = double sine of half that arc, the chord ia = double the sine iv, or the sine av = sine iv, and the arc Ai = the arc Aa. Draw aH, joining a, the upper extremity of the sine av, with H, the termination of the quadrant AH; and parallel to aH draw vm, connecting the lower extremity v, of the sine av, with m, the termination of the quadrant cnm described with half radius. Join aF = radius of the given circle. Bisect the sine av in p, and parallel to the diameter AB draw pno, passing through the point n of intersection of the radius aF with the line vm.

Because aF = radius of given circle, and nF = half radius; therefore an = half radius or = nF. And because the sine av is bisected in p, and pn is perpendicular to av, the sides ap, pn, of the triangle apn are respectively equal to the sides vp, pn, of the triangle vpn, and they include equal angles apn, vpn. Consequently the side vn = the side an, or = nF. The line pno being parallel to the diameter AB, the arc nc = the arc oD, and the co-arc nm = the co-arc mo. Consequently the $\angle\, n$Fc = the $\angle\, o$FD. And because the triangle vFn is isosceles, and the side vn = the side nF, the $\angle\, nv$F = the $\angle\, n$Fv = the $\angle\, o$FD, or by sim. $\Delta s.$ = the $\angle\, m\,n\,o$. But the $\angle\, m\,n\,o$ at the circumference = only half the $\angle\, m$Fo at the centre. Therefore the arc mo (= the arc nm) is double the arc oD, or double the arc nc. See Leslie's Geometrical Analysis, book i. prop. 31st, prefixed to his Geometry of Curve Lines. Or, since the line po, which bisects the sine av in p, bisects its equal mF in w, the $\angle\, mnw$ or $\angle\, mno$ = $\angle\, n$Fc; because the sine mw of the former $\angle$ is = the sine wF of the latter $\angle$. But because the $\angle\, mno$ is an angle at the circumference, the arc mo, or its equal the arc mn, is double the arc nc, the $\angle\, n$Fc being an $\angle$ at the centre. Again, because the circumferences of circles have the same ratio to each other as their radii, and the radius AF, or aF, is double the radius nF or cF, the arcs Aa and aH are respectively double of the arcs cn and nm, and the arc aH is double the arc Aa: or this latter arc Aa is = $\frac{1}{3}$d of the arc AH, or = $\frac{1}{3}$d of HB.

In a similar manner we may find the third of any fractional part of the quadrant, as well as a third of the whole, because for every variable position of the chord HL we describe a new circle with the varying radius cL. That is, when HB ceases to be the arc of a right angle, or, which is the same thing, when HC moves into the position of Rc, or when, instead

stead of a right angle, the given arc becomes only = RB, then cL varies to cq, which now becomes a substituting radius for cL.

To find one third part of the arc AE, or of RB.—Draw the line RtFr through the centre F of both the concentric circles. Through the point c, which bisects the radius AF, draw the chord Rcq, meeting the given circle in q. With the radius qc, and centre c, describe the lunar arc qPd, intersecting or meeting the given circle in the points q and d points equidistant from P, the extension of the diameter AB. To prevent a confusion of sines, we will pass to the other side of the diameter AB. Through the point of intersection d draw the augmented sine dh. Apply the chord of the arc Ad to the arc Pd (as for the quadrant) from P to c, and through this latter point c draw the sine cg, intersecting the given circle in the point u. The sine ug is the true sine, or is the sine of $\frac{1}{3}$d of the arc AE, or of $\frac{1}{3}$d of the arc RB, and consequently the arc Au = $\frac{1}{3}$d of the arc AE, or $\frac{1}{3}$d RB. Make the arc Ae = the arc Au, then will the arc Ae = $\frac{1}{3}$d of the arc AE, or = $\frac{1}{3}$d of RB. If the upper extremity u of the sine ug were connected with R, the termination of the arc RB, by means of a straight line; and if a straight line were drawn from g, the lower extremity of the sine ug, to t, the termination of the arc Dt; and if, moreover, the sine ug were bisected in a manner similar to that employed for the right angle, a similar proof would follow; viz. that the arc Au = $\frac{1}{3}$d of the arc AE, or = $\frac{1}{3}$d of the arc RB. The straight line drawn from g would fall on the arc cn between c and n, and would be = vn, or = nF, and on being produced would pass through the point of intersection made by the other two lines; drawn from u, and from the bisection of the sine ug.

Since the arc Aa = $\frac{1}{3}$d of the arc AH, or = $\frac{1}{3}$d of HB, and the arc Au = $\frac{1}{3}$d of the arc AE, or = $\frac{1}{3}$d of RB; the difference between these thirds, viz. the arc ua, or the arc ei, = $\frac{1}{3}$d of the co-arc EH, or of HR. Make, next, the arc iQ = the arc Ai, then will the arc eQ = $\frac{1}{3}$d of the arc EB, or = $\frac{1}{3}$d of the arc AR.

Scholium.—When the proposed arc is less than half a quadrant, as the arc EH or HR; the complement AE, or RB, may be trisected, and the difference between $\frac{1}{3}$d of this complement, and $\frac{1}{3}$d of a quadrant as the arc ua, will be = $\frac{1}{3}$d of the proposed arc EH or HR.

P. N.

V. *On*

V. *On a Method of observing Solar Eclipses by means of the Altitude and Azimuth Instrument.* *By* A CORRESPONDENT.

To the Editors of the Philosophical Magazine and Journal.

A DAY or two ago, after perusing Mr. Troughton's able defence of the altitude and azimuth instrument, it struck me that it might be advantageously employed at the end (and probably at the *beginning*) of the next solar eclipse to ascertain the moon's declination and right ascension, as well as the longitude of the place of observation.

The instrument being carefully *orienté*, observe the azimuth (from the north) and the zenith distance of the point of contact, and correct the latter for refraction.

With this datum, the calculated zenith distance of the sun's centre affected by parallax, and his semi-diameter (diminished by irradiation?), find the angle formed at the sun's centre between the zenith and the point of contact. This angle, together with the (*parallaxed*) Z. D. of the sun, and the sum of the *apparent* semi-diameters, will give the moon's azimuth and (*parallaxed*) zenith distance.

Admitting the earth to be a spheroid, the difference of the true and apparent zeniths, together with the azimuths and zenith distances, afford data to diminish* the former and increase the latter to the quantities due to the reduced latitude.

The parallaxes being subtracted from these transposed zenith distances, we find (as in lunars) the distances of the centres of the sun and moon, and consequently the longitude of the observer. The method of deducing the N. P. distance of the moon, as well as the right ascension, is sufficiently obvious.

The apparent time of the end of the eclipse being known to great accuracy, it might serve to calculate the azimuth of the sun's centre; and consequently the semi-diameter. By comparing the two methods, we might learn the value of the irradiation.

An observer in possession of a well regulated chronometer furnished with a micrometer (or merely with vertical and horizontal wires) might arrive at the same results by comparing the point of contact with the proper parts of the sun's disk.

June 21, 1823. X. X.

* The objects being to the *north* of east.

VII. *True*

VI. *True apparent Right Ascension of Dr.* MASKELYNE'S 36 *Stars for every Day in the Year* 1823, *at the Time of passing the Meridian of Greenwich.* [Continued from vol. lxi. page 435.]

1823.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel.	β Tauri	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 53	H. M. 4 25	H. M. 5 3	H. M. 5 6	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 30	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 40	H. M. 11 41	H. M. 13 15	H. M. 14 7
Aug.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	11·57	16·16	4·97	48·82	40·43	3·84	9·00	37·36	21·89	19·28	3·51	30·23	54·52	57·85	3·45	30·56	55·22	37·97
2	59	19	5·00	85	47	87	03	38	91	30	53	25	52	85	44	56	21	96
3	62	23	03	88	50	89	06	41	93	32	54	27	53	85	44	55	20	94
4	65	26	06	92	54	92	09	43	95	34	56	29	54	85	43	55	19	93
5	67	29	10	95	58	95	13	46	98	36	58	31	54	86	43	54	18	91
6	70	32	13	98	62	98	16	49	22·00	38	59	33	55	86	42	54	17	90
7	72	35	16	49·01	66	4·00	19	51	02	41	61	35	56	86	42	53	16	89
8	74	39	19	04	70	03	22	54	04	43	63	37	56	87	41	53	15	87
9	76	42	22	07	74	06	25	57	06	45	65	39	57	87	41	52	14	85
10	79	45	25	10	·78	08	28	59	08	48	67	41	* 58	88	40	52	13	84
11	81	48	28	13	82	11	31	62	11	50	68	44	60	88	40	51	12	82
12	83	51	31	16	86	14	35	65	13	53	70	46	61	89	39	51	11	81
13	85	54	34	19	90	17	38	67	15	55	72	48	62	89	39	50	10	79
14	87	57	37	23	94	20	41	70	17	58	74	51	63	90	38	50	09	78
15	89	60	40	26	98	22	44	73	20	60	76	53	64	91	38	50	08	76
16	92	63	43	29	41·02	25	47	75	22	63	78	55	65	91	38	49	07	75
17	94	66	46	32	06	28	51	78	24	65	80	58	66	92	37	49	06	73
18	96	69	49	35	10	31	54	81	27	68	82	60	67	93	37	49	05	72
19	98	72	52	38	14	34	57	84	29	70	84	62	68	93	37	49	04	70
20	12·00	75	55	41	18	37	61	87	32	73	86	65	69	94	37	49	03	69
21	02	78	58	45	22	40	64	89	34	76	88	67	70	* 96	37	49	02	68
22	04	81	60	48	27	43	68	92	37	79	91	70	71	97	37	48	01	66
23	05	83	63	51	31	45	71	95	39	81	93	72	73	98	37	48	00	65
24	07	86	66	54	35	48	74	98	42	84	95	74	74	99	36	48	54·99	64
25	09	89	69	57	39	51	78	38·01	44	87	97	77	75	58·00	36	48	98	63
26	11	92	72	61	43	54	81	03	47	89	99	79	76	01	36	48	97	61
27	13	94	75	64	48	57	85	06	49	92	4·02	82	78	02	36	48	97	60
28	15	97	78	67	52	60	88	09	52	95	04	84	79	03	36	48	96	59
29	17	17·00	81	70	56	63	91	12	54	98	06	87	80	04	36	48	95	58
30	18	02	84	73	60	66	95	15	57	20·01	09	89	82	05	36	48	94	56
31	20	05	86	76	65	69	98	18	60	03	11	92	83	06	36	48	94	55

1823.	1α Libræ.	2α Libræ.	α Cor. Bor.	α Serpentis.	Antares.	α Herculis.	α Ophiuchi.	α Lyræ.	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1α Capri.	2α Capri.	α Cygni.	α Aqua.	Fomalhaut.	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 56	H. M. 23 59
Aug.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	57·92	9·35	14·63	36·52	38·28	38·24	46·77	60·10	54·60	12·75	41·15	54·27	18·10	27·58	45·49	55·54	0·85	19·01
2	90	33	62	51	26	23	76	09	60	75	15	27	10	58	51	56	87	04
3	89	32	60	50	25	22	75	08	60	75	15	28	11	58	52	59	89	07
4	88	31	59	48	24	21	74	07	60	76	15	29	12	59	54	61	91	09
5	87	30	57	47	23	20	73	06	60	76	15	29	12	59	55	63	92	12
6	86	29	55	46	22	19	72	05	60	76	16	30	13	59	57	65	94	15
7	84	27	54	44	20	18	71	04	60	76	16	30	13	59	58	67	96	17
8	83	26	52	43	19	17	70	03	60	76	16	31	14	59	59	69	98	20
9	82	25	50	42	18	16	69	02	60	76	16	31	14	59	60	71	1·00	23
10	80	23	49	41	16	15	68	01	60	76	16	31	14	59	62	73	02	25
11	79	22	47	39	15	13	67	59·99	59	76	16	31	14	59	63	75	03	27
12	78	21	45	38	14	12	65	98	59	76	16	31	14	59	64	77	05	30
13	77	20	44	37	12	11	64	97	59	76	16	31	14	58	65	79	07	32
14	75	18	42	36	11	10	63	95	59	75	16	31	14	58	66	80	08	34
15	74	17	40	34	09	08	62	94	58	75	16	31	14	58	67	82	10	37
16	73	16	39	33	08	07	61	92	58	75	16	32	15	58	68	84	11	39
17	71	14	37	32	07	06	59	91	58	75	16	32	15	57	69	85	13	41
18	70	13	35	30	05	04	58	89	57	74	15	32	15	57	70	87	14	43
19	69	12	33	29	04	03	57	87	57	74	15	32	15	56	71	89	16	45
20	67	10	31	27	02	01	55	86	56	73	14	32	15	56	71	90	17	47
21	66	09	30	26	01	00	54	84	56	73	14	32	15	55	72	92	18	49
22	65	08	28	24	37·99	37·98	53	82	55	72	13	31	14	54	73	93	19	51
23	64	07	26	23	98	97	51	81	55	72	13	31	14	54	73	94	21	53
24	62	05	24	21	96	95	50	79	54	71	12	31	14	53	74	95	22	55
25	61	04	22	20	95	94	49	77	53	70	11	30	13	52	74	97	23	57
26	60	03	21	18	93	92	47	75	53	70	11	30	13	51	75	98	24	59
27	58	01	19	17	92	91	46	73	52	69	10	30	13	50	75	99	25	60
28	57	00	17	15	90	89	44	71	51	68	09	29	12	49	76	56·00	26	62
29	56	8·99	15	13	88	87	42	69	50	67	08	29	12	48	75	01	27	64
30	55	98	13	12	87	85	40	67	49	66	07	28	11	47	76	02	28	66
31	53	96	12	10	85	84	39	65	48	65	06	27	10	46	77	03	29	67

N.B. On those days where an Asterisk is prefixed the Star passes twice; the Æ given is that at the first passage.

VII. *On Metallic Titanium.* *By* W. H. Wollaston, *M.D. V.P.R.S.**

THE evidence that we yet possess of the reduction of titanium to its metallic state, is not altogether satisfactory; for even Laugier (who has described a valuable series of experiments made upon it in 1814, and who had the advantage of all the previous knowledge acquired by the labours of Vauquelin and Hecht in 1796, of Lowitz in 1798, and of Lampadius in 1803) could only say that he thought himself justified in considering certain parts of his product which were of a golden colour as really reduced; adding in confirmation, that Messrs. Vauquelin and Haüy, to whom he had shown them, "appeared disposed to adopt his opinion †."

As M. Laugier had not the means of confirming his opinion by analysis, I may presume that an account of some experiments which I have recently made upon this substance will be acceptable to chemists in general; and that in proportion to the degree of doubt they may entertain, they will feel interested to examine scrupulously the evidence I shall adduce as to the metallic state of the subject of my experiments.

My attention has been directed by various friends, especially by Professor Buckland, who gave me the subject of my experiments, to certain very small cubes, having the lustre of burnished copper, that occasionally are found in the slag of the great iron-works at Merthyr Tydvil, in Wales, which, from their hue, have by some persons been imagined to be pyritical. Their colour, however, is not truly that of any sulphuret of iron that I have seen; and though the form be cubic, it is not the striated cube of common iron pyrites, which so often passes into the pentagonal dodecahedron, but similar to that of common salt; for any marks, that are to be discerned on their surfaces, appear as indented squares instead of striæ.

Their hardness also is totally different from that of pyrites, and is such as, when combined with the preceding characters, marks a substance wholly unknown to mineralogists. By selecting a sharp angle of one of these cubes, I found that I could not only write upon the hardest steel, or upon crown glass, but could even visibly scratch a polished surface of agate or rock crystal.

* From the Philosophical Transactions, Part I. for 1823.

† Je me crois fondé à regarder cette couche mammelonnée comme la portion réellement réduite ....
MM. Vauquelin et Haüy m'ont paru disposés à adopter cette opinion.
Annales de Chimie, vol. lxxxix. p. 317.

Having

Having broken out some of these crystals for experiment, I found them all apparently attracted by a magnet; but observing that they had still small portions of slag adherent to them, they were next digested in muriatic acid, which, by dissolving the iron from their surfaces, soon freed them from their deceptive appearance of magnetism.

The cubes thus purified are not acted upon by muriatic acid.

Nitric acid has no action upon them.

Nitro-muriatic acid does not dissolve them.

Boiling sulphuric acid does not affect them.

Before the blow-pipe they are utterly infusible. A continued heat oxidates them, and they become purple or red at the surface, according to the degree of oxidation, or depth to which it penetrates.

Borax has no action upon them, but only cleans the surface from any oxide that may be formed. Neither does the addition of subcarbonate of soda produce more effect than borax alone.

Nitre, aided by a strong heat, oxidates them rapidly: but unless the heat be long continued, the effect is only superficial.

The combined action of nitre and borax together soon effects their solution, as the latter dissolves the oxide as fast as it is formed, and presents a clean surface for fresh oxidation. But as these salts do not unite by fusion, the addition of soda, as a medium of union, considerably shortens the process. The fused mass becomes opake in cooling, by the deposit of a white oxide, which may either be previously freed of the salts by boiling water and then dissolved in muriatic acid, or the whole mass may be at once dissolved together.

In either case alkalies precipitate from the solution a white oxide, which is not soluble by excess of alkali, either pure, or in the state of carbonate. By evaporating the muriatic solution of the oxide to dryness, at the heat of boiling water, it is freed of any redundant acid, and the muriate which remains is perfectly soluble in water, and in a state most favourable for exhibiting the characteristic properties of the metal.

Infusion of galls gives the well known colour of gallate of titanium. The colour occasioned by adding triple prussiate of potash is red, as observed by Laugier, and so nearly resembling that of the gallate, that I do not think any difference that I can discern is to be depended upon as constant. It differs from prussiate of copper by inclining to orange instead of purple, while the colour of prussiate of uranium is rather brown than red.

 Since

Since the oxide thus examined agrees in its characteristic properties with that of titanium procured from Anatase, I cannot entertain a doubt as to the general nature of the substance under consideration. I believe it to be pure, for I find no trace of any other substance combined with it, not even of iron, although the crystals are found imbedded in an iron slag, in the presence of metallic iron; nor yet of silica, for which the oxide has a strong affinity. Neither is there any sulphur present, as the salt which remains after oxidation of it by nitre, contains no trace of sulphuric acid.

That the cubes are in the metallic state, is nearly proved by their lustre, by the effect of nitre upon them, and by the failure of borax to act upon them, till they have been subjected to the action of nitre. It may be further observed, that, when the action of nitre is rapid, heat is evidently generated, as by the combustion of other metals: but as I acted upon them in their solid state, and did not pulverise them, I did not witness what could properly be called detonation, as described by Lampadius.

The property which may be regarded as most decisive of the metallic state of these cubes, is the power which I find them to possess of perfectly conducting the most feeble electricity.

If a slip of zinc and another of copper be placed in contact, and immersed together in dilute sulphuric acid, bubbles of gas are seen to rise from the surfaces of both the metals; but, if a piece of paper be interposed between them, then no gas is given off by the copper. In a piece of paper, so placed between zinc and copper, I made a small hole, and after inserting in it one of the cubes so as to be in contact with both the metals, I had the satisfaction to find an electric communication completely established by this interposition, for gas was now given off from the surface of the copper.

From the situation in which this metal is found, it evidently has no affinity for iron in the metallic state, and it seems equally indisposed to unite with every other metal that I have tried. Though it is evidently impossible to measure with precision the specific gravity of such specimens as I first received for analysis, I was in hopes of trying whether one of the largest of the cubes would sink or swim in melted tin, and for that purpose endeavoured to tin its surface; but I could not succeed in uniting it with either tin or lead, with silver or copper, and had no encouragement to prosecute further a series of negative results, in search of metals for which it may have an affinity.

From the extreme infusibilty of these cubes, it seems probable

bable that they have not been formed by crystallization in cooling from a state of fusion, but have received their successive increments by reduction of the oxide dissolved in the slag around them: a mode of formation to which we must have recourse for conceiving rightly the formation in nature of many other metallic crystals.

Since the date of this communication, the liberality of Mr. Anthony Hill, of Merthyr Tydvil, has supplied me with a larger quantity of the slag which formed the subject of my first experiments, and has enabled me to determine the specific gravity of metallic titanium to be 5·3. For this purpose, the vitreous part was fused with a mixture of borax and subcarbonate of soda in about equal quantities, and was then dissolved in muriatic acid, which also removed a quantity of metallic iron, and left the titanium freed from extraneous matter. Though great part of what was thus obtained from the interior of the slag was in a pulverulent state, the quantity, which amounted to 32 grains, and displaced 6·04 of water, was sufficient to preclude any considerable error.

I have moreover learned that metallic cubes, similar to those which I have above described and examined, were more than 20 years since observed in a slag at the Clyde iron-works in Scotland; that a small quantity has also been met with at the Low Moor iron-works, near Bradford in Yorkshire; and at the Pidding iron-works, near Alfreton in Derbyshire; and that some good specimens have been obtained from Pontypool in Monmouthshire; but it does not appear that any one has ascertained or even suspected the real nature of this singular product.

VIII. *Report of* Thomas Telford, *Esq. on the Effects which will be produced on the River Thames by the Rebuilding of London Bridge.*

In consequence of the authority given me by the resolution of the Committee for letting the Bridge-House Estates, dated the 7th of March last, I immediately took measures to get an accurate survey made of the river, its banks and appendages. For this purpose I employed two persons experienced in making similar surveys, viz. one for the district from London Bridge to Putney, and the other from Putney to Teddington Lock; and in order to ensure accuracy and proper connexion and uniformity, I caused one of my own assistants, also accustomed to river surveys, to carry levels from London Bridge to Teddington Lock, and I have myself superintended

perintended and occasionally inspected the proceedings: I have also received the tidal observations made at different times at several stations upon the river.

In order to proceed with regularity, I shall adopt the following arrangement in tracing the effects which would be produced to the westward and also to the eastward of London Bridge, if the present edifice, which constitutes a dam of from 1 foot 1 inch to 5 feet 7 inches, or 4 feet 4 inches on an average, were removed, and in its stead a new bridge, with comparatively little obstruction, were substituted:

1st. Observations on the comparative state of high water, founded on the surveys and levels lately taken, and the tidal observations made in 1820, 1822, and 1823; and further, what is likely to take place if London Bridge be removed.

2dly. Similar observations as regards the state of low water.

3dly. As to the effects which the aforesaid changes are likely to produce upon the navigation, bridges, banks, wharfs, shores, and adjacent properties.

First, As to the State of the River at High Water.

It appears from the table of observations of the height of the tides at the several bridges in 1820 and 1822, that the average fall through London Bridge at high water was from 8 to 13 inches; that by those of 1823, since the removal of the water-works, the fall instead of 8 inches is now only from 3 to 4 inches; I think therefore it is fair to conclude that with a still less obstructed waterway there will be little or no fall at high water, and that hereafter high tides in the western parts of the city will even in calm weather be at least on the same level as below bridge. I find that the level of the wharfs below bridge is from $2\frac{1}{2}$ to 4 feet above the Trinity datum, and that those of $2\frac{1}{2}$ feet are occasionally flooded. The average level of the wharfs above bridge is from $1\frac{1}{2}$ to 2 feet above the Trinity datum; and the extraordinary flood of 1821, which rose at Teddington 7 feet, rose at Putney only 2 feet, and at Lambeth 1 foot 11 inches above the said datum. Therefore it appears that there is more reason at present to dread the elevation arising from the tide below bridge, than from floods above; and that the floods of the Thames are not sufficient, in the present state of things, to fill the lagoon or pond above the narrows of the bridge to the height which some of the tides do below, and which, there is reason to believe they also would above, were the channel unobstructed.

But it may be supposed that the quantity of tide coming in at the Nore being given, the additional space provided for it by

by opening the upper part of the river will prevent it from rising so high as it now does near the bridge, and that therefore not only is there a probability of no greater elevation occurring there than at present, but that it will, in similar circumstances, be lower below bridge—consequently that no danger can arise above. To this I reply, that when it is high water at the Nore, we have it, within two hours, high water at London Bridge, at the distance of forty miles; so that the high water passes up at the rate of twenty miles per hour: so much more rapidly than any known velocity of the river, that its effects are not to be accounted for by the flowing of the current merely, as may be supposed the case in filling up the pond to Teddington through the arches of London Bridge.

In this last case we have levelled along the banks of the river, and find, after correcting the marks expressing Trinity datum, that the lowest surface of high water is between Putney and Kew; that it rises about one foot to Teddington, and nearly as much at London Docks: but this is liable to considerable variation. The rise in the upper part of the river pond may be easily accounted for by the accumulation of the fresh waters of the river over and above what is tidal water. The fall from London towards Putney seems to show that the tide has not time, through the contracted passage, to fill up the pond above bridge to the lower level.

From London Bridge to Blackwall the high water seems, from the observations, to be level: the quantity of water required to fill up this difference of level is, after all, so small, that with an unobstructed waterway it would evidently make no difference worthy of notice in the level of the tide below bridge, even were it subtracted from the mass that lies between London Bridge and the Nore. Whereas considering the great rapidity with which the lower part of the river is filled by the tide, it is clear that an unobstructed tide would fill up this trifling increase in at least as little time as the present period.

But to render this a matter of calculation: we find the average breadth of tide-water at the Nore to be $3\frac{1}{2}$ miles; at Gravesend half a mile, the distance being 18 miles; which, at 6000 feet per geographical mile, with 15 feet of tide, gives from the Nore to Gravesend 17,000 millions cubic feet of tide-water: at London Bridge, taking the breadth at 1000 feet and 3000 at Gravesend, we have in 24 miles, and with the same depth, 4320 million of cubic feet, or 1-4th additional tide-water. There run at present through London Bridge, between the lowest ebbs and high water of ordinary springs (or 14-feet tides) above bridge, 582 millions cubic feet (582,342,710); and

and if London Bridge be removed, so that there be no material dam at low water, we have also to fill the pond now caused by that dam. This pond is from 4 to 6 feet deep at the bridge, at low water; and we find that the level of low water above bridge meets the bottom of the Thames between Putney and Kew, viz. 10½ miles above bridge: taking this as the head of the pond, the average breadth at 600 feet at low water, the mean depth to be filled at 2 feet, we have an addition of 75 millions of cubic feet, or 1-57th of the quantity of tide-water between London and Gravesend, or only 1-284th of the whole quantity of tide-water within the Nore; therefore the whole water which must pass the New Bridge, to raise the upper river to the level of high water below bridge, is 657 millions, or 1-32d of the entire quantity of tide-water within the Nore below bridge.

It is a well-known fact that the tide in narrow channels with funnel-shaped mouths, or against coasts which oppose its regular course, rises considerably higher than at the places which are situated in retired bays, or under the wake of projecting points: thus the Atlantic tide running up the Channel rises 6 or 7 fathoms against the French coast near St. Malo and Havre; while on the opposite English coast, at Portland and Poole, we have only one fathom rise. In St. George's Channel, the tides at Milford and along the Welsh coast rise four fathoms; on the opposite Irish coast, from Carnsore Point to Wicklow, hardly one fathom. Many similar instances might be given. Again, as to funnel-shaped mouths: the spring-tide at the entrance of Bristol Channel rises 22 to 24 feet; but as that channel contracts in breadth, the velocity and vertical rise increase in proportion so much, that in King Road it rises between 7 and 8 fathoms. Many other similar instances may be shown. As may be perceived by the position of the banks of the Thames' mouth, the flood-tide comes from the N.E. or German sea: at half-past eleven it is high water at Harwich, Kentish Knock, and Margate; the oscillation or rise at springs is from 15 to 16 feet; at twelve it is high water at the Nore; and although the rise there is only 14 feet, yet in the Swale, which is in the direct course of the tide, the rise is 17 to 21 feet at half-past twelve.

The general set of the current running up the Thames forms a branch which at the Nore at noon rises, as we have said, 14 feet; but from thence the funnel-shape produces a gradual increase in the oscillation until we arrive near London: that at Gravesend, at one, the rise is 16 feet; at Woolwich, at three-quarters past one, it is 18 feet; at Deptford, at two o'clock, we have 18¾ feet; but at Billingsgate, at a quarter

past

past two o'clock, there is a rise of $17\frac{1}{2}$ feet only. The action of the tide is now affected by the bridges, the regular progress of this wave being checked, and the surface of the high water declines all the way to Putney, where it is high water at a quarter past three o'clock; but from thence again there is a rise of one foot to Teddington, where it is high water at three-quarters past four. Hence observe that from Billingsgate to Teddington the wave passes at the rate of 8 miles per hour only; while below Billingsgate the same wave of high water passes at the rate of 20 miles per hour, or more particularly

	Miles.		Time. Hour.	Minutes.		Miles.
From the Nore to Gravesend	18	in	1	0	is	18 per ho.
Woolwich	15	—	$\frac{3}{4}$	0	—	20
Deptford	$6\frac{1}{2}$	—	$\frac{1}{4}$	0	—	26
Billingsgate	4	—	$\frac{1}{4}$	0	—	16
Swan Stairs, a loss of				10	—	
Putney	7	in		50	—	$8\frac{1}{2}$
Teddington	11	—	$1\frac{1}{2}$	0	—	$7\frac{1}{3}$

It is obvious then that this rapid diminution of the velocity of high water is caused by the narrow at London Bridge, and that, were that obstruction removed, there is every reason to believe the velocity in the upper river would be greatly increased.

It must also be observed that the fall or difference of height between the surfaces above and below bridge at high water must not alone be taken as the proper measure of the obstruction, and used as a datum throughout a calculation, because the fall through the whole tide is much greater. In one very moderate spring-tide, which I observed on the 26th of May last, when the fall at high water was only 5 inches, the fall through most of the preceding part of the tide had been 14 inches.

The high water will therefore go up to the head of the tide-way more speedily, and will rise higher than at present.

Secondly, Of the River at Low Water.

This water must also return with greater velocity, and the removal of the bridge will not only permit the increased head to pass off at the ebb, but likewise that portion which is now retained by the obstruction.

Were the flood tide not to return, and the stream of the river to cease, the bed would exhibit a series of ponds at levels, gradually increasing in elevation as we pass to the westward; of which the first would extend to Battersea Bridge, having a shoal at Westminster Bridge, on which there will be little or no

no water, and nearly 2000 yards in length. The second pond, from Battersea to Putney, would be 16 inches higher than the former. At Putney Bridge would be a rise of 17 inches. Above Putney to Mortlake is a shallow channel with small pools; in the deepest passage across the bars there is now less than 3 feet of water. Mortlake is the next pond, two miles in length. Its surface is level with the present low water at London Bridge; but before the construction of that work it would, as its name implies, have been a dead or stagnant lake at low water. The other ponds which are higher than the present low water may be observed in the general section. The depth over the bar is no where less than $2\frac{1}{2}$ feet, or more than 4 feet; but this depth is with some difficulty sufficient at present for navigation to the locks at Teddington.

Were the river water to be run off above bridge, this navigation must cease, unless a new channel be excavated through the shoals: independent of the depression in the lower pond which the New Bridge will permit, a longer time will be given for the ebb to empty the upper reaches, as we may see by inquiring whether the obstruction of London Bridge occasions any remarkable deviation from the progress of the ebb, as we have just found it to do in the case of the flood tide, whereby we form some judgement of the probable result of its removal with respect to the velocity of the ebb stream.

Allowing therefore that the tide at the Nore occupies 6 hours 16 minutes, or the regular half tide, we find that low water proceeds —

		Miles.		Time. Hour.	Minutes.	Miles.
From the Nore to	Gravesend	18	in	1	24	13 per ho.
	Woolwich	15	—	1	8	
	Deptford	$6\frac{1}{2}$	—	0	$37\frac{1}{2}$	$10\frac{2}{3}$
	Billingsgate	4	—	0	$22\frac{1}{2}$	10
	Old Swan, a loss of				20	
	Westminster	2	in	0	$22\frac{1}{2}$	5
	Putney	$5\frac{1}{2}$	—	1	34	$3\frac{1}{2}$
	Teddington	11	—	3	20	$3\frac{1}{3}$

which exhibits the same rapid changes of velocity caused by the bridge as in the case of flood.

Were the bridge removed, therefore, it is evident that the velocity of ebb above bridge would materially increase, the time of low water be earlier than at present, the drainage of the upper ponds more complete, and the navigation which is now practicable up to Teddington would cease too early near that place.

Thirdly, Effects to be produced.

And lastly, from the foregoing statement of facts it has been shown

shown that the removal of London Bridge will admit a greater body of water to flow up the river to the westward, and with a greater velocity, which together will considerably increase the momentum; and it is equally certain that the same cause will operate in the ebbing tide, and leave the bed of the river nearly dry for several hours in the latter part of the ebb. This will in part be remedied by the increased velocity and momentum scouring away the mud, sand, and small gravel, so as to deepen the bed; but this cannot take place where the matter has more consistence, and to obtain the same depth as at present at low water would require excavation to a very great extent, probably to incur an expense of 40,000*l.*

But this lowering of the bed, if accomplished either by the tide scour or artificial excavation, would seriously affect the foundations of some of the other bridges. The piers of Westminster Bridge stand upon gravel without having piles under them, and several are now not more than 3 feet under the present surface of the river bed, the matter of which I proved to be sand and gravel. By the plate of the geometrical elevation and plan of Blackfriars' Bridge, published from drawings by Mr. Baldwin, the bottom of the platforms is not more than about 5 feet below the present bed of the river: these piers have, it is true, piles of about 10 feet in length under them, but if the bed were lowered they would require to be protected. Some of the piers of Waterloo Bridge have their platforms laid only at about 6 feet 4 inches under the line of the present low-water mark. Respecting the bridges between Westminster and Teddington, which stand partly on stone piers and partly upon wooden piles, I have not hitherto been able to obtain any accurate information; but it is clear that the lowering of the bed of the river would in some measure affect them.

With regard to wharfs and houses built on the banks of the river, the lowering of the surface of low water, and extending the time of that depression, would afford an opportunity of a greater drainage from the adjacent soil upon which buildings are erected, and may have the effect of causing settlements: if no excavation takes place in the shores adjacent to the wharfs, the barges, &c. will be longer prevented from approaching to or departing from them: if an excavation does take place, there will be some risk of the walls being undermined. These observations apply to the whole river as far as Teddington.

Besides these consequences from lowering the bed of the river, others will unavoidably follow from the tide above London Bridge rising higher than it does at present. Many of the wharfs by the sides of the river are not more than from $1\frac{1}{2}$ to 2 feet above Trinity Datum, and are not unfrequently

 overflowed,

overflowed, partly by land floods, but chiefly by high tides, which rise above a foot higher below bridge than they do at present above bridge: the evil will therefore be proportionably increased both in degree and frequency. But besides the common operations of land floods and tides in calm weather, all the river above bridge will, when the dam is removed, be further exposed to the influx of heavier waves driven from the Nore, with storms from the northward, which have hitherto been checked by the almost solid mass of the upper part of London Bridge. These observations apply to all the banks and low grounds on each side of the river from Westminster to Teddington, and which are very extensive.

Instances of such influx and rising of the tide have been already mentioned, and another has come to my knowledge while engaged in the present survey. At the Cashen river in Kerry, which falls into the sea near the mouth of the Shannon, a bar has been lately cut across to make a more direct navigation: the upper river has thereby been lowered two or three feet at low water, and at high water raised so as to overflow the marshes more than before; and the direct stream is now cutting a channel through the sandy shoals above the bar. This information I received from the able engineer (Mr. Nimmo) who advised the measure.

The Effects Eastward of the Bridge.—No longitudinal or cross sections having been taken to the eastward of the bridge, I have no accurate knowledge of the state of the river bed, and can therefore only observe generally, that my investigations have led me to the conclusion that more water will pass with a greater velocity in every part of the river; but as the difference will diminish as the section increases, the effects will of course disappear in the lower parts of the river. When operations do take place, they will scour and deepen the river, where the matter is alluvial and loose.

24, Abingdon-street, Westminster, June 11, 1823. THOMAS TELFORD.

IX. *Observations on the Project of taking down and rebuilding London Bridge*.*

IT is a matter certainly of great interest to men of science, to know what effect the removal of a dam producing a fall of water westward at high water sometimes of two feet, and eastward at low water sometimes of nine feet, from a great river like the Thames, would have westward and eastward of that dam in respect to the bed and shores of such a river; and

* From the Quarterly Journal of Science, &c. No. XXX.

whether

whether a more frequent inundation and saturation with water of the low lands will cause miasms and pestilential diseases again to prevail, should the means of stopping such inundations or of quickly draining off the water not be immediately obtained. They look forward with great anxiety to the experiment; and the knowledge that this dam has existed many centuries, that the river passes through a dense population, that the estates of individuals have been regulated by it, that the levels of the lowest floors of houses and those of the streets in the low lands adjacent, have reference to this habit of the river, adds much to the excitement; for the intenseness of the interest always increases with the hazard of the throw. The complaints of the inhabitants on the banks of the river, like those of the dumb creature subject to the knife of the surgeon, are not heard in the eager pursuit of knowledge, and in the speculation of future amelioration. There are others who have great influence, and are urgent for the demolition of London Bridge, looking to their own gain* in the erection of a new one. A mathematician, like to him of Laputa, has brought his implements to the question, and, without sections, without levels, and ignorant of the soil over which the river flows, or against which it impinges at its sinuosities, knowing neither what may be overflowed, nor what may be sapped, has, by a kind of intuitive philosophical tact, determined that, after the removal of the dam, the stream will flow on as harmless and obedient as heretofore†. Presuming there may be some of your readers unable to discover truth except by induction, and others costive of their belief in the delirations even of a great teacher, and thinking that they may be desirous of viewing this important question by any glass, however weak its power, your correspondent ventures to offer that by which he views the question, and solicits the shelter of a few pages for the following observations in your journal.

The writers on the ordinances of rivers consider the courses and velocities of them dependent on the nature of the ground over which they pass, as well as upon the heights from which their waters descend. For example: water descending from a height on rocky ground, which it cannot remove, rises, spreads, and forms a lake; and proceeds with diminished velocity to the lowest point, and there cascades; advancing at the rate of forty-five inches per second, it will drive flint stones about the size of an egg before it, and rise and spread until its velocity

* There is no doubt that that writer here ascribes the efforts which have been made with so much success to procure the demolition of London Bridge to their real cause; although the influence of a Committee of the House of Commons was employed for the attainment of the object.—Edit.

† See Dr. Hutton's Answers, App. 4th Report, 1821.

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is reduced to thirty-six inches per second, when the stones remain at rest: proceeding among pebbles about an inch diameter, it serves them the same, rising and spreading until its velocity is reduced to twenty-four inches per second, when they remain at rest: proceeding through coarse gravel about the size of a marble, it serves it the same, rising and spreading until its velocity is reduced to twelve inches per second: and so it proceeds with diminished velocity according to the size of the grain, the velocity and the course always varying with the obstacles met with. Gravel, the grain being about the size of aniseed, will be at rest at a velocity of four inches per second. Sand will remain at rest at a velocity of seven inches per second, and precipitate at six inches per second. Clay will remain at rest at a velocity of three inches per second*. By reference to the map of the river Thames west of London Bridge, and bearing the above-mentioned facts in mind, it will appear that the banks of the river from Nine Elms, a little above Vauxhall Bridge, to London Bridge may be considered artificially fenced, and only requiring additional aid by raising and wharfing to prevent overflowing and sapping, through any increased height and velocity of the current; and, consequently, as the waters will not be allowed to spread in a neighbourhood where land is so valuable, the bed of the Thames in this part must be deepened naturally if the current acquires increased velocity; and, therefore, the bridges, in this part, especially Vauxhall and Westminster Bridges, which do not stand upon piles, must be secured. If, proceeding from Fulham and impinging on the shore of Wandsworth and Battersea†, the water should find the soil less resistive than on the opposite bank of the Grove, Chelsea, and Ranelagh, and the banks be not artificially strengthened, the water may take a short cut at some high flood in its course to the sea, from Fulham to Nine Elms, and place Battersea in Middlesex. The same principles will apply both to the effects of the flood and ebb tides, from an increased velocity, at the several bendings of the stream; and, without expensive wharfings and continual care after the dam is removed, the proprietors of lands on the river shores, where there are elbows, may expect sometimes to lose a rood, and sometimes an acre of their lands, together with their sheep and cows.

The present turbidness of the river, and the frequent shifting of some of the banks and shoals, show it to be now sometimes

* See *Principes d'Hydraulique*, par M. le Chev. Du Buat; *Expériences sur les Cours des Fleuves*, par M. Genneté; and the article *River*, Ency. Brit.

† The river here is comparatively rough and rapid. The boatmen have a story, that a band of fiddlers at this place were in former times drowned, and that the river has been dancing here ever since. Another band are determined to make the land join in the jig.

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at variance with its bed and banks. Hence it is necessary to ascertain the nature of the soil of the bed of the river and of its banks at the several points of sinuation up us high as Tide-end-town, wherever it may be hereafter, whenever there are buildings to be sapped *; and this inquiry should be made in the survey, which, by an extract from the Report of Mr. Telford in the Phil. Mag. of May last, he has requested authority to get made, complaining that no such document exists; the persons examined before him since 1800 up to this session of parliament, as to the effect likely to be produced by the enlargement of the water-way of London Bridge, *having been able to decide* upon these matters without the data Mr. Telford now thinks necessary. Such a river as the Thames, which, at a mean width between London and Blackfriars Bridges, even now the dam exists, having a velocity in the mid stream of sixty-three † inches per second, or $3\frac{6}{10}$ miles per hour, at half flood, requires some respect to be paid to its speed, its windings, and its fences, and will be found indignant to an alteration of its ancient habits. The paradoxes which experiments on the flowing of waters present, the recent history of the Eau Brink as to its anticipated and its actual effect on the harbour of Lynn, the erroneous calculations of the Royal Academy of Paris in respect to the apparently simple question of the Paris aqueduct, and those of Desaguliers and MacLaurin as to that of Edinburgh, might cause some doubt of any opinion with sufficient data, *and much more of the determinations of mere theory*, from one of very advanced age, without any. The question relating to the effects of the removal of the dam westward, put in the following manner, would cause more inquiry than the present seems to have done.

What effect would the introduction of another river on the west side of London Bridge, of the same dimensions as the river Thames at London Bridge, with a fall into it of two feet, have upon the bed and banks westward at high water? What effect would the subtraction of a quantity of water, at low water, equal to the surface of the river, six feet in depth at that subtraction, have upon the river westward at that time of the tide? It has been maintained, with reference to a compensation clause in the bill for the new bridge, that, in cases of land-floods, the removal of the dam of London Bridge would not cause an in-

* See Appendix (A. 23, 3d Report. Lond. Port.) in which are given the bornings from London to Blackfriars' Bridge, from which it appears that the bed of the river in that part is gravel and sand, coarse and fine.

† See 3d Report, Appendix G. London Port, and Plate 20, Appendix. At Westminster, Mr. Labelye ascertained the velocity to be thirty-six inches per second.

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creased height of the waters in the up country, but have a contrary effect. This position is true at all times of the ebbing, but not of the flowing; a high sea-flood meeting a high land-flood must dam back the latter, and at times two feet higher than at present, when the dam of the bridge is removed. For example: on the 28th of December 1821, from the freshes, the whole of the up-country was so flooded that the inhabitants of the low-lands adjacent used boats in the streets; a sea-flood meeting such a flood, and suffered to rise two feet higher than it can at present, would have caused a greater extent of country to be flooded than suffered at that time*.

Those who favour the removal of the dam of London Bridge, should, during the present hot weather, take a boat at low water from London Bridge, and proceed up the river; and, whilst they enjoy the odour from the banks, contemplate the effects of lowering the water from four to six feet, consequent on such removal, occasionally requiring the boatman to sound the depth with his oar; it will then be manifest to them what a stinking ditch the river will become at low water. Though an expenditure of a large sum of money might dredge out a temporary channel for the navigation at that time, it must nevertheless be remembered, that the width of the river increases upwards from London Bridge, and there are no moveable dams, for which purposes the ships below London Bridge are used to keep it clear. The cause assigned for taking down London Bridge is as follows: "Whereas the great fall of water at certain times of the tide, occasioned by the large starlings and piers of the said bridge, renders the navigation through the

* The late Mr. Mylne's Report, Appendix (A 1) and Plate I, 3rd Report, London Port, without data, but from a practical tact, confirms the opinions contained in this paper. He was employed with a view to the demolition of London Bridge, and was a strenuous advocate for a new one. He contemplates the inadequacy of the sea-walls, but leaves, like the new bill, the care of them to the respective owners. If we may rely on the effect of the increased velocity on the bed of the Thames, which he anticipates, there will, soon after the dam is removed, be the materials of two or three bridges ready wrought at London Bridge for the new structure, without the trouble of stopping the receipts of the excise and customs of the three kingdoms. The fall of water, westward of London Bridge, has dug out the bed of the river, to a distance of four hundred feet, of twenty-eight feet in depth at the lowest point; and that eastward from the ebbing and freshes, has dug out the bed of the river to a distance of six hundred feet, of thirty-four feet in depth below the bed at the lowest point: when the dam of the bridge is removed, this power will be principally spent in deepening the river upwards. The maintaining Blackfriars Bridge, even with the present bed of the river, ought to be more an object of solicitude than the destruction of London Bridge; its piers are in a very dilapidated state,—and it is to be remembered that the piles under them were not driven nor cut off within coffer-dams.

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said bridge dangerous and destructive to the lives and properties of His Majesty's subjects*." By reference to the Reports of the Committees of the House of Commons, of the sessions 1820 and 1821, relating to this bridge, ordered to be printed May and June 1821, and upon abstracting from the evidence therein, relating to the loss of life and property in the last twenty years, the promoters of the demolition of the bridge cannot produce a statement of a greater number of persons drowned than 17, nor damage to property exceeding 4000*l.* by accidents at London Bridge during that time. The evidence, with respect to the danger of the navigation through the bridge, of the lightermen examined, many of whom have navigated the river for forty years, is directly at variance with the opinions of those who are desirous of a new bridge, and attributes the accidents which occur, to mere ignorance and drunkenness.

The sufficient stability of this bridge was ascertained in 1759, when the large arch was made, and unquestionably confirmed by the late examination of the structure of the piers †.

The sufficient width of the bridge as a roadway, is maintained by Mr. Rennie's evidence (16th April 1821), who, upon being asked, "What would you propose to make the width of the new bridge?" answered, "The same width as the old one;" and added, London Bridge is wider than either Southwark, Blackfriars, or Waterloo Bridges. The width of the bridge, in the clear of the parapets, in the design which received the first premium, is only 44½ feet, a less width than between the parapets of the present bridge‡; so that the mechanics and tradesmen who urge the necessity of a new bridge, in the hope of having a *freer thoroughfare* for themselves and their carts, *will be grievously disappointed.*

In the late application to architects and engineers, it seems remarkable, that it had not occurred to the Bridge Committee, that the supposed evil might have another remedy than a new bridge; and out of the course of ordinary proceeding. It might have suggested itself to some engineer, contemplating the di-

* The passion for legislating about London Bridge is not new, although it now has changed its direction. In the last century Parliament passed an act to compel the corporation to *stop up* some of the arches, and to *increase* the fall which the present act complains of.—Edit.

† Appendix, Report on London Bridge, 1821, p. 66, &c.

‡ See Mr. Dance's section, Append. B. I. 2d Report, London Port. By Append. B. III. 3d Report, London Port, London Bridge is 45 feet wide, Blackfriars 41 feet, Westminster 39 feet 9 inches.

The late Mr. Mylne (App. B. II.) thought 50 feet a proper width for the new London Bridge. The roadway of Waterloo Bridge is 28 feet, the footpaths each seven feet, together 42 feet; the same as Westminster Bridgei s stated to be by Mr. Labelye. Vauxhall Bridge has a roadway of 28 feet, and two footpaths of 5 feet 6 inches each, together 39 feet.

rection of the mid stream of the Thames towards Pepper-alley stairs, and the bank of gravel that directs it in that course; or to some antiquary, who recollected King Canute's mode of conveying his fleet from the east side to the west side of London Bridge; or the direction of the cut which was made in 1173, when this bridge was rebuilt,—that an auxiliary cut and bridge, round the foot of the present structure, north of Tooley-street, might be a cheaper mode of obtaining the proposed object than a new bridge; especially upon finding, upon inquiry, that between the linear waterway (690 feet) required, and the absolute linear waterway of the present bridge (545 feet), there is only a deficiency of 145 feet; and between the superficial waterway of London Bridge, and that of the section of the whole river, from Old Swan-stairs to Pocock's Flour wharf, at high water, there is only a deficiency of about 4000 feet.

Others, deprecating the removal of the dam, but desirous of rendering the navigation, even when intrusted to unskilful and drunken lightermen, safe, and accustomed to view the locks on other rivers, and even upon this, may surmise, that the object might be obtained by locks *.

It appears, that there are about 750,000*l.* in embryo for the new bridge, squaring, of course, with the estimates; but, upon referring to the bill brought into parliament this session, for rebuilding London Bridge, there seems to have been originally some doubt as to the sufficiency of means †; for it will be found, that the Commissioners of His Majesty's Treasury were to be allowed to issue exchequer bills for the approaches, and they were also to be allowed to pay the expenses of the act, and direct taxes were to be levied on the public, *on coals and wine* imported into the city of London, for liquidating and paying the interests of these exchequer bills, under

* Had the instructions to these candidates been unfettered, there might have been a renewal of Messrs. Douglas and Telford's scheme for a cast-iron bridge of 600 feet span, with a rise of 65 feet above high water, for vessels to sail above London Bridge, and only at the cost of 262,289*l.* The practicability and advisableness of this bridge was certified by twelve out of fifteen mathematicians and engineers, though, at that time, neither the designers, nor the committee,nor any of the mathematicians or engineers, knew the strength of cast-iron; and those who supposed they knew something of the matter, thought it forty times stronger than it since has been found to be: so easy is it to ask and receive opinions. But where a favourite object is to be carried, the data, upon which such opinions must be founded, are kept out of sight or mis-stated, or an inquiry into them is refused.

† The amended bill makes the doubt approach to a certainty; for it is said to contain a specific clause, that no one shall be entitled to compensation for any nuisance, obstruction, or injury, on account of the bridge remaining unfinished, in case the sum or sums of money, to be raised and advanced, prove insufficient to complete the same.

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the screen of what is called the Orphans' Fund, and *indirectly*, *by the introduction of a clause to exempt the corporation "from the payment of any damage to persons, or their houses, estates, vessels, or property*, by reason of the increased rise of the tide of the said river above the said bridge, or the alteration of the channels or currents of the said river, or of the want of water for navigating the same, nor for any *nuisance, obstruction, or injury*, to be occasioned thereby*."

But it being understood that the direct taxes might be indigestible, that part of the bill is struck out, and a less visible mode of taxation is to be adopted, by allowing the Commissioners of Customs and of Excise, of England, Ireland, and Scotland, with consent of the Lords of the Treasury, to remit taxes on stone, brick, timber, or other materials used in building the bridge, and its *appurtenances*. For this purpose, the ordinary course of Government is to stop, and there is to be a particular interposition; but the poor people, who may be ruined in their fortunes, diseased by the damps and miasms caused by the saturation of their habitations by frequent floods, or overwhelmed by floods, from an inability to provide against them, consequent on this revolution of the ancient and now constitutional habit of the river, are left to the care of a higher Power, who has set his bow in the heavens as a token. The scheme seems now to be†, to pass the act and get up the bridge, relying, in the case of a deficiency of money to rebuild it, that Government would be compelled, by the urgency of the occasion, to provide the means. Such a scheme, in respect to the Post-office, failed: but that was a singular case, an exception to the general success of such policy.

The new bridge, proposed by the late Mr. Rennie, was estimated by him to cost		£430,000
A temporary bridge		20,000
The purchase of property for approaches	On the north side	150,000
	On the south side	150,000
		£750,000

This sum, by reference to absolute costs, compared

* Those who have built their houses low in the low-lands, and feed their cattle there, the proprietors, and others, who have allowed the foundations of their bridges to be laid at an insufficient depth, are informed that they came to the river, and not the river to them; and that they ought, in choosing such a neighbour, to have provided against such an event as the proposed alteration of the habits of it.

† It would have been but justice to state that this scheme is not to be imputed to the corporation, which has from the first remonstrated against the destruction of London Bridge, as a project set on foot by interested persons. And to the assertions of those who declared their conviction, from what had come to their knowledge of the proceedings of the Committee of the House of Commons, that the whole was a job, no answer has ever been given.—Edit.

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with estimates of other works of the same kind, might with propriety be taken as half the cost, even could we not see the causes from which such an excess would arise, viz. at £1,500,000

But we have the following items* of charge, by which we may guess that doubling the estimate will be found too small an allowance for contingencies.

1. The bridge is to be erected in a hole where the depth of water, at high water, is 46 feet.
2. The approaches are to be made through property of great value, and in a thoroughfare of persons and carriages as close as sheep in a flock.
3. On removing the old bridge.

* Many great losses will be sustained by individuals under the heads of these items, but for which they will be shut out from having any compensation from the City; nevertheless they must be considered part of the cost of the new bridge. It may be proper to inquire, who are to be subject to these actions, suits, indictments, claims, and demands, which are thus shifted‡ from the mayor, commonalty, and citizens? On the northern shore, we find, among others, the Duke of Northumberland, the Rev. William Lowth, the Duke of Devonshire, the owners of Fulham Town Meadow, Viscount Cremorne, Lord Cadogan, Lord Grosvenor, the Chelsea Water-works Company, the Crown, and others.

From Teddington eastward to Cotton stairs, near Westminster Bridge, all the river walls are defective in height to resist such a flood as that of the 28th December 1821, that deficiency varying from one foot at Twickenham, to two feet five inches at Cotton Garden stairs; but, generally, in the less populous parts westward, the walls are from three to five feet below that level; while the lands in the populous parts northward are greatly below it: for example, Walham-green and Chelsea are from one to five feet below this level. The ground of the Penitentiary is eight feet below this level. The Vauxhall Bridge road, and Tothill-fields, are generally from three to four feet below this level. St. James's Park, on the south side, varies from sixteen inches to eight feet below this level; and there are various defective banks or ways, as far eastward as the Duchess of Buccleugh's, for the water to get to these parts. It will be the duty of the commissioners of sewers forthwith to give notice to the various proprietors to repair their banks, by raising or otherwise; and it will be a matter determinable by the custom or peculiar laws of the commissioners, whether, in default of complying with such notices, the commissioners may direct the proper raisings and wharfings to be done, and rate the proprietors of the banks for the cost, or leave them to the actions, suits, indictments, &c., of which the mayor and commonalty are so apprehensive‡.

After the demolition of the dam of London Bridge, this level will be that of not a very uncommon high sea-tide west of London Bridge.

‡ As this undertaking is *forced upon* the mayor, commonalty and citizens in spite of their almost unanimous opinion, repeatedly expressed, it was but justice that they should not be made liable for the damage which may be sustained by the neighbouring proprietors. The injury which it is apprehended will be done to the navigation and to the corporate property may be a sufficient share of loss for them to bear.—Edit.

4. On

4. On raising about 40 miles of river wall, varying from 24 to 26 inches in height, and strengthening the banks by wharfing and piling, in order to provide against the effects of frequent floods, expectant on giving a freer water-way, and increased velocity and height, to the current.
5. On dredging out a channel for the current at low water, for the navigation.
6. On the necessity of narrowing the river in several parts.
7. On removing shoals and sand-banks, caused by the alteration in the directions of the mid stream.
8. On the erection of starlings round the piers of the different bridges, and especially round Vauxhall and Westminster Bridges, which do not stand upon piles. The bridges above London Bridge generally stand in shallow water, and the foundations of them are very little below the bed of the river, which may be undermined; for a greater depth must be effected artificially, in the first instance, for the navigation, and subsequently, by the increased velocity of the stream, in a manner which cannot now be guessed at*.
9. On the necessity of erecting another dam, or locks, to keep up the water, as a substitute for the dam taken down, the necessity for which, the locks up the river, beginning at Teddington, prove †.
10. On the damage to shipping below the bridge, in times of frost, by ice now stopped, at such times, by London Bridge.
11. On compensations to persons possessed of wharfs, adapted to the present state of the river above and below the bridge, for damage to them by the alterations in the course of the stream, and the shifting of the sand banks.
12. On compensation to persons whose trades are dependent on the free thoroughfare over the bridge, living south and north thereof, for seven years, during the erection, or while it remains unfinished for want of funds to complete it.

* The head of water maintained by the lock at Teddington in winter is one foot, in summer four feet; a similar head is maintained at Moulsey. Dams are erected here to keep the water up the country; but the dam of London Bridge is to be taken down to let it out.

† The bottom of the foundations of the piers of Westminster Bridge is five feet below the bed of the river, allowing two feet three inches, as at Blackfriars Bridge, for grating; the bottom of the stone is only two feet nine inches below the bed. The bottom of the foundations of the piers of Blackfriars Bridge is three feet nine inches below the bed, the bottom of the stone eighteen inches. How much below the bed of the river are the foundations of Vauxhall, Waterloo, and Southwark Bridges? The bottom of the stone piers of Waterloo Bridge is only fifteen feet below the springing of the arches.

13. On

13. On compensation to persons navigating the river, for property destroyed, and loss of life, during the erection of the bridge, and while it may remain unfinished for want of money to complete it, which, at a moderate estimate, may be taken to exceed the same loss arising from the old bridge in the last twenty years.

Hence, in any view of the question, it would be unreasonable to consider the cost of this bridge at less than *one million and a half.*

These observations may probably, through your Journal, cause more inquiry to be made into this important question, than the *impatient deter ination, at any rate to have a new bridge*, has hitherto a ved. They may make the failure of the proof of the expedie y of removing the dam of the bridge manifest; also show the deficiency of the means for building the bridge, without taxes to a large amount being eventually levied on the public; and remove the general delusion, that the thoroughfare over the bridge will be more free than it is at present. They may cause some reflections on the forbearance of the Government regarding the public dignity, but scrupulous of increasing the public expenditure, in listening for a moment to such an useless and dangerous expense, which, directly or indirectly, will cause taxes to be raised to pay a million at least."

X. *An Account of the Observations and Experiments on the Temperature of Mines, which have recently been made in Cornwall, and the North of England; comprising the Substance of various Papers on the Subject lately published in the Transactions of the Royal Geological Society of Cornwall, and other Works.*

[Continued from vol. lxi. p. 447.]

IV. MR. R. W. FOX's third communication on this subject to the Cornish Geological Society "was unfortunately too late for insertion in the second volume of Transactions, a circumstance which the editors very much regret," in a note attached to the ninth Annual Report of the Society's Council, "because the facts and observations therein contained form an important addition to the papers on that very important subject. The Council rejoice, however, that an opportunity for its publication is now afforded, and that it will form the leading article in the first number of the Society's Transactions, to be printed before the next anniversary." The substance of this communication, however, we are enabled to present,

present, from the Annals of Philosophy for December last, p. 440.

"The high temperature which prevails in mines having excited some attention, I am induced to submit to the Cornwall Geological Society, the result of further observations, which have been made on the subject in several mines since my last communication." (Phil. Mag. vol. lxi. p. 350.)

"At South Huel Towan Copper Mine, in the parish of St. Agnes, the temperature of the water in the cistern at the "*sump*," or bottom of the mine (45 fathoms deep), was 60°. This may be taken therefore as the mean temperature of the streams of water which flow through the deepest levels, or galleries, into the cistern.—Two men were employed at one time, that is, 8 in 24 hours in this part of the mine."

"East Liscomb, a copper mine in Devonshire; depth 82 fathoms; temperature of water in the cistern 64°."

"Huel Unity Wood, a tin and copper mine in Gwennap parish; depth 86 fathoms; temperature of water taken as before, 64°. Four men constantly worked at the bottom of this mine."

"Beer Alston, a lead mine in Devonshire; 120 fathoms deep; water 66·5° of temperature, taken as before."

"Poldice, a tin and copper mine in the parish of Gwennap; temperature of the water 78° in the lowest cistern in one shaft, which was 144 fathoms deep.—Eight men were constantly employed at a time at the bottom of this part of the mine, besides two men during the day ("*on tribute*"). The temperature of the water in another shaft of the same depth, and tried in the same way, was 80°: two men only were employed at a time in the levels at the bottom."

"Consolidated copper mines in Gwennap. One shaft is 150 fathoms deep, and the temperature of the water 76°: six men were employed at a time at the bottom. The temperature of the water, ascertained in the same way, in another shaft of the same depth, was 80°; and here there were eight men at work at a time."

"Huel Friendship, a copper mine in Devonshire. Temperature of the water taken as above, was 64·5° at the depth of 170 fathoms. The number of men employed at the bottom has not been reported; but as they were sinking the engine shaft, there could not be less than two. There is, when its depth is considered, a very small quantity of water flowing into the bottom of this mine; for it requires only a six-inch box, and five strokes of the engine a minute to draw it up. The mine is situated on very elevated ground bordering the granite hills of Dartmoor. Although the temperature of the water is probably more than 14° above the mean of the climate in

in which it is situated, it is certainly much inferior to the temperature generally observed in mines of the same depth."

"The undermentioned mines being partly filled with water, I give the temperature of the water remaining in each."

"North Huel Virgin, a copper mine in St. Agnes parish. The temperature of the water, which stood at 39 fathoms under the surface, was 60°."

"Nangiles, a copper mine in the parish of Kea. The temperature of the water, at 59 fathoms under the surface, was 58°. Nangiles is 88 fathoms deep at the engine-shaft. The machinery for pumping the water out of this mine had very recently been set to work, and had consequently made but little progress in draining it. I mention this, in connexion with my remarks on the temperature of stopped mines, in order to account for its not being greater. The veins in this mine are large, and remarkable for the quantity of iron pyrites they contain."

"Tresavean, a copper mine in Gwennap. The temperature of the water, standing at 100 fathoms under the surface, is 60°; and the whole depth of the mine is 170 fathoms. It is situated on elevated ground, about 480 feet above the level of the sea, and is moreover in granite, in which the temperature generally appears to be inferior to what is observed in 'killas,' or clay slate, at equal depths."

"Huel Maid copper mine. The water which it contains is 126 fathoms from the surface, 30 fathoms in depth, and 60° of temperature. There are no pumps in this mine; but the water has recently been considerably reduced, in consequence of the reworking and draining of some neighbouring mines: all the water from the higher levels &c. must therefore be raised with that in the mine, and reduce its temperature; which is in a considerable degree prevented in mines which are furnished with pumps, by placing cisterns to receive the water at different levels."

"Mines which contain much water, if the workings have been only recently renewed, are generally of an inferior temperature to those of equal depth, which are drained to the bottom. This remark applies, in a much greater degree, to mines which have been long stopped and filled with water; in confirmation of which the three following instances may be given."

"The water in Herland copper mine, in the parish of Gwinear, in the shaft, at the adit-level, 31 fathoms deep, is only 54°, though the mine is 161 fathoms in depth."

"At South Huel Ann, in the same parish, the water in the shaft was likewise 54°; the depth of the adit being 11, and that of the mine 23 fathoms."

"At Gunnis Lake copper mine, in the parish of Calstock, which is 125 fathoms deep, the water in the shaft, at the adit-level 35 fathoms deep, was 57°."

"The water that flows out through the adits of stopped mines, is, I presume, derived from the superincumbent strata, or indirectly, by displacing the water in the shafts, or in the upper levels that communicate with them, and which must be in a greater or less degree more accessible, and offer an easier outlet to the water, than those which are deeper and more remote. If this be admitted, it follows that the water which issues out of the tops of shafts of stopped mines, does not proceed from the *deeper* levels; but, on the contrary, it seems highly probable that the water they contain is nearly stationary, and, as it does not readily communicate heat in a lateral direction, that its temperature may materially vary from that in the shafts; whereas it is well known that in a perpendicular or oblique column of water, an interchange will take place between the warmer part of the liquid column at the bottom and the colder at the top, till an equality of temperature is produced through the whole."

"I attribute the higher temperature of the water in Gunnis Lake shaft, at least in part, to the very elevated ground in its immediate neighbourhood; although the relative temperature of the water in the shafts of stopped mines may also depend on the greater or less depth at which the columns of water commencing above the adit-level communicate with the shafts, or with the levels connected with them."

"When the working of Tincroft tin and copper mine, in Camborn parish, was recently resumed, after it had been for several months suspended, an opportunity occurred for ascertaining the temperature of the water, when it was sunk to the depth of 126 fathoms under the surface, and was only 10 fathoms deep, in the bottom of the mine. It was then found to be 63°; and this was before many men had resumed their labours, or indeed any of them, at the inferior levels; and moreover, at the time of making the observations, even the few men who worked in the mine had not been in it for the space of nearly two days. Near the middle of 1819, when the water stood at the same place in the mine, and it was, and had long been, in a state of full working, the temperature of the water at the bottom was only 59°. Perhaps the water will again be reduced to this temperature, if it should remain at the same depth in the mine; for is it not reasonable to suppose, that the droppings of colder water down through the shafts, must affect the temperature of that at the bottom?"

"In consequence of an accident in the steam-engine at

 Ting-

Ting-Tang, the water rose considerably in the mine. On its being again sunk to within 10 fathoms of the bottom, the mine being 117 fathoms deep, its temperature at this station was found to be 63·5°; whereas the water pumped up from the bottom, into a cistern immediately above the place of observation, was 65°: so that the water seems to have been 1·5° warmer at the depth of 10 fathoms, than at its surface. This phenomenon must, I think, be attributed to the under current from the levels caused by the action of the pumps."

"A fact, communicated to me by a gentleman in the brewhouse of Barclay and Co. at Southwark, may here be noticed. Not long ago, a well was sunk in order to procure water for the supply of the brewery. They did not attain their object until they had got down 140 feet under the surface, and cut through the great bed of clay which lies under the metropolis. The water then rose rapidly in the well, its temperature being 54°, which it invariably maintains at all seasons of the year. Now the climate of London and its vicinity is at the mean temperature of 49·5° on the authority of Luke Howard, which is 4·5° under that of the water in the well."

"I stated at the last annual meeting of this Society (Phil. Mag. vol. lxi. p. 353) that a thermometer buried at the depth of 3 feet in a rock, in a level at Dolcoath mine, 230 fathoms under the surface, indicated, during eight months, a temperature of about 75° to 75·5° when the mine was clear of water. It has subsequently remained in the same place nearly twelve months longer, and the mercury has continued stationary at 75·5°, notwithstanding the changes of the season."

"Although I think it will be admitted, that the bottoms of our mines are, for many reasons, less liable to be influenced by adventitious causes than the superior levels, I shall give the results of various observations made on the temperature of water in the undermentioned mines, at different levels, and, as far as it was ascertained, of the air also at the same stations; in order to show the relative temperature of both, and the ratio in which it increased in depth, without particularizing the mines in which the experiments were respectively made; as this appears to be unnecessary."

"The mines referred to were South Huel Towan, East Liscomb, Huel Unity-Wood, Beer-Alston, Poldice, the Consolidated Mines, Huel Friendship, the United Mines, Treskerby, Huel Damsel, Ting-Tang, and likewise Huel Maid, Nangiles, North Huel Virgin, and Tresavean. The four last-mentioned mines having been partly full of water for many years, the figures which refer to them are distinguished by an asterisk."

Depth

Depth from the surface in feet.	Depth from the surface in fathoms.	Medium in which the observations were made.	Degrees of Temperature.							
60	10	water								
		air								
120	20	water	58°							
		air	56°							
180	30	water	55	64						
		air	56	58						
240	40	water	56	60	54	*54	*56	*56	*60	
		air	57	60	60	*55	*57		*60	
300	50	water	60	60	60	60				
		air			58	58				
360	60	water	60	62	60	58	*58			
		air	61	62		60	*57			
420	70	water	61	*58	*58					
		air	61		*59					
480	80	water	64	64	62	*59				
		air	65	64	64					
540	90	water	64	66						
		air	62	67						
600	100	water	65	*60	*60					
		air	66		*56					
660	110	water	64	66	64					
		air	65	66						
720	120	water	66½	66						
		air	68	68						
780	130	water	66	72	74	74	63	72	68	*60
		air		73	74		62	73	70	*58
840	140	water	78	70	80	72				
		air	78	72	81	75				
900	150	water	76	80						
		air	72	80						
960	160	water	66							
		air	73							
1020	170	water	64½	77	84					
		air	66	76						
1080	180	water	72	69	86	87				
		air	74	72	88					

"In

" In taking the temperature of the water in the different levels of mines, care was generally observed to select the largest streams, and to put the thermometer at or near the places where they first flow into the mines, so that the influence of any heat from the miners seems to be put out of the question."

" It appears that in almost all the mines which have been examined, the highest temperature has been found at the bottom; and it is deserving of notice, that here, in most instances that I have investigated since my last paper, very few workmen are employed; and generally their number increases at each level in ascending from the bottom, as high up as one-quarter or even one-third of the way; so that not very far from the middle of mines they are frequently the most numerous. At a level 180 fathoms under the surface, in the United Mines, I find the temperature of the water, which was, and had been during twelve months, 30 fathoms deep in the mine, was 80°, and a stream of water flowing into the same level was 87°. This is only half a degree less than it was at the same place in 1820. At that time, about 400 men were employed in the mine 8 hours each day, and about 50 on an average for the remainder of the 24 hours. When the last observation was made, only about 200 men worked in the mine 8 hours a day, and about 50 during the remaining 16 hours."

" I do not dispute, that in close levels, where there is no current, the presence of the men increases the temperature of the air; yet it does not appear by the above table that the heat of the air is usually much greater than that of the water in the same places,—perhaps on an average not exceeding 1° or 2°. In many instances, indeed, the water was from 1° to 4° warmer than the surrounding air, and this occurred in several mines at or near the deepest levels."

" Before I conclude my enumeration of facts, it may perhaps be desirable to state the temperature of the water which flows through the great adit, and is discharged near Nangiles mine, above Carnon Valley. This adit traverses the principal mining district of Cornwall, and extends nearly 30 miles, including its different ramifications, and more than 5 miles from one extremity to the other in one direction, and 3 miles in another. The temperature of the water was taken near the mouth of the adit about six weeks since, and was found to be 69·25°. Richard Thomas, land surveyor, of Falmouth, (author of an interesting map of a large portion of our mining district,) has ascertained by frequent observations, that the quantity of water discharged by the adit, at different times of the year, has varied from 910 to 1644 cubic feet per minute: but

but as some deep mines have been set to work since he made his experiments, the average quantity is now probably greater. It appears, on making a comparison of the depth of the water at the time the foregoing temperature was ascertained, with his calculations, that the quantity discharged was equal to 1400 cubic feet per minute, or about 60,000 tuns per day."

"The great adit is divided into three principal branches, the first of which unites with it at about a mile from its mouth, and communicates with the United and the Consolidated Mines, Huel Squire, Ting-Tang, Huel Maid, and South Huel Jewel; the average depth of which mines seems to be about 150 to 160 fathoms. The temperature of the water in this branch, near the junction, and about 1½ mile from the mines which principally supply it with water, was 73·5° about the end of last month, when this and the following observations were made. At nearly a mile further on, the great adit is divided into two branches; one of them receives the water from Poldice, Huel Unity, Huel Unity-Wood, Huel Damsel, Huel Pink, Rose Lobby, Huel Hope, Huel Gorland, Huel Jewel, and Huel Clinton; the average depth of which is perhaps from 110 to 120 fathoms, and the temperature of the water in the branch, at about a mile from the principal mines above named, was 66·5°. The other branch is connected with Treskerby, Huel Chance, Chacewater, North Downs, Cregbraws, Huel Boys, Cardrew, and a few smaller mines; their average depth may be estimated at 100 to 110 fathoms, and the temperature of the water in the adit, about 3½ miles from the mines, was 65°. I have not ascertained the quantity of water discharged by each of these branches; but it is evident they carry off, not only the water pumped from the various levels of the respective mines, but also that which is drained from the strata under which they pass, and which is from 30 to 50, and in some places from 60 to 70 fathoms in thickness."

"The temperature of the water in the adit is therefore even more considerable than might be expected; and the difference observed in the branches may be attributed to the relative depths of the mines with which they are connected, and to many of those communicating with the two last-mentioned branches, being stopped, or partly full of water."

"I have mentioned that the water flows into cisterns at different levels in mines, being partly or entirely retained by the rock on which it rests; but generally, from the strata being more or less porous, some of the water sinks through it, and may either mix with an inferior portion before it flows into the levels, or it sometimes descends in numerous drops or small streamlets

streamlets from the roofs of deeper levels; and in either case, it must produce more or less influence on the temperature, and prevent its being uniform at equal depths. If there were a perfectly free and open communication between the various portions of water under the surface of the earth, it is evident that mines could not be drained, but the pressure of the columns of water would be irresistible, and their impetuosity overwhelming."

"The high temperature in mines seems to have no necessary connexion with the minerals which they contain: even where iron pyrites is very abundant, the heat does not appear to be greater than where it is the reverse."

"Having recently tried some experiments on the water taken from the bottom of several deep mines, I find it in most instances to contain in solution a very minute quantity of any foreign substances, varying perhaps from one to five or six grains in a pint. Its relative purity appears to have no reference to the depth or temperature of the mines; for instance, Huel Abraham and Dolcoath are the two deepest and two of the warmest mines in the county, and the water from the bottom of these mines does not in either case hold in solution more than about two grains of foreign matter in a pint. On the other hand, some mines abound with much less pure water: that from the Consolidated Mines leaves ten grains of residuum from a pint; Huel Unity, sixteen grains; from one shaft in Poldice, nineteen, and from another *ninety-two* grains, from the same quantity. In most of the mine-water that I have examined, the muriatic salts, especially the muriates of lime and of iron, are most abundant. I have detected *muriate of soda* in some instances, particularly in the water from the bottom of the United Mines, the Consolidated Mines, Huel Unity, and Poldice."

"Out of the 92 grains of residuum, produced from a pint of water from one of the engine shafts of the latter mine, 24 grains proved to be *muriate of soda;* 52 grains the muriates of lime and magnesia, chiefly the former; and the remainder muriate of iron, and a small quantity of the sulphate of lime. The water from another engine shaft of the same mine contained 5½ grains of muriate of soda, and about 13 grains of the muriates of lime and magnesia, and the carbonated oxide of iron."

"All the mines above enumerated are situated in the interior of this part of Cornwall, and are distant several miles from the sea!"

[To be continued.]

XI. *The*

XI. *The Third Portion of a Catalogue of Zodiacal Stars for the Epoch of January* 1, 1800; *from the Works of* Herschel, Piazzi, Bode, *and others; with illustrative Notes. Selected and arranged by a Member of the Astronomical Society of London.*

Constellations: *Orion, Auriga, Gemini, Cancer.*

Synonyms. P.	B.	F C.M.	Character.	Constellation.	Mag.	VI hours. Right Asc. m.	°	′	″	A.V +	Declination + °	′	″	A.V. −	Lat. °
2	269	68	(E.1)	Ori. †	6	0	90	2	33·0	53·24	19	49	17·5		—3·7
3	18	6		Gem.†	6·7	0		2	47·4	54·40	22	56	22·5		—0·5
7	271	69	f. 1	Ori. †	6	1		7	46·5	51·75	16	9	45·3		—7·3
8	270	70	ξ	——	5	1		8	30·0	51·16	14	14	24·9	0·10	—9·2
13				Gem.†	8	2		28	46·5	54·95*	24	1	37·9	0·17*	0·5
18	200	44	κ	Aur. †	4	3		39	22·9	57·24	29	33	22·3	0·55	6·1
22	20	7	η	Gem.†	4·5	3		42	1·2	54·25	22	33	2·5	0·25	—0·9
				Ori. †	7·8	3		43·8			14	32·1			—8·7
23	276	71	(E.2)	——	5·6	3		46	13·5	53·02	19	12	38·6	0·27*	—4·3
24				——	8	3		47	59·1	51·80*	16	4	42·0	0·28*	—7·4
29	279	72	f. 2	——†	6	4		58	13·9	51·86	16	11	30·7	0·22	—7·3
30	23	8		Gem.	7	4	91	1	26·1	54·82	24	1	14·3	0·36	0·5
33	25	9		——†	7	5		11	35·7	54·78	23	47	37·1	0·43	0·3
43	26		(t)	——	7	6		27	10·5	56·34*	27	16	22·9	0·51*	3·8
51	27	10		——	7·8	7		40	44·7	54·71	23	39	58·6	0·59	0·2
52	28	11		——	7	7		47	6·9	54·69	23	32	2·6	0·58	0·1
53	29	12		——†	8	7		48	9·0	54·66*	23	20	22·6	0·61	—0·1
62	30	M. 248		——	8	9	92	19	7·5	53·78*	21	12	26·1	0·81*	—2·2
64	31	M. 249		——	8	9		21	0·0	53·81*	21	16	31·2	0·82*	—2·2
67	32	M. 250		——	8	10		24	13·5	54·86*	23	50	20·2	0·84*	0·4
74	33	13	μ	——†	3	11		42	49·9	54·43	22	36	8·5	1·05	—0·8
78	34			——	7	12	93	6	6·0	55·40*	25	8	21·2	1·09*	1·8
87		C. 183		——	7	13		20	30·9	54·72*	23	32	13·4	1·17*	0·1
89	36	C. 184		——	7	13		20	48·3	54·68*	23	25	22·8	1·17*	0·0
91	37	14		——	7·8	14		25	33·7	53·91	21	44	31·6	1·19	—1·7
94				——	8	14		35	9·0	51·07*	14	11	26·4	1·25*	—9·2
98	225	48	(z)	Aur.	6	16		55	38·7	57·84*	30	36	7·0	1·35	7·2
100	38	15		Gem. †	6	16		57	46·3	53·57	20	53	53·1	1·43	—2·5
101	39	16		——	6	16	94	0	40·8	53·48	20	36	11·0	1·39	—2·8
				——†	7	16		3			21	16			—2·1
109	42	18	ν	——	5	17		16	13·5	53·37	20	19	30·8	1·46	—3·1
114	228			Aur.	7·8	18		26	8·4	56·78*	28	19	49·0	1·55*	4·9
120				Gem. †	8·9	19		41	39·3	53·49*	20	32	29·5	1·64*	—2·9
				——†	8	19		43·8			21	55·5			—1·5
126	231			Aur.	7	19		50	54·9	58·78*	32	34	55·5	1·69*	9·3
129				Gem.	8	20	95	0	16·5	51·73*	15	58	47·7	1·75*	—7·4
130	44	19		——	6·7	20		1	44·7	51·74	16	1	50·0	1·71	—7·4
135	45	20		——†	7	21		9	36·9	52·40	17	54	41·2	1·76	—5·5
				——†	8	22		30·0			22	15·4			—1·0
142	236			Aur.	8·9	22		31	16·5	58·27*	31	34	25·0	1·93*	8·3

Synonyms.			Character.	Constellation.	Mag.	VI hours. Right Asc.					Declination. +				Lat.
P.	B.	F.C.M.				m.	°	′	″	A.V.+	°	′	″	A V −	°
144	48			Gem. †	6·7	22	95	33	37·5	51·10*	14	17	44·1	1·94*	—9·1
146	237	49	(c)	Aur.	6	23		39	3·7	56·83	28	9	47·2	2·01	4·8
147	47	22		Gem.	7·8	23		42	40·2	52·99	19	34	9·7	1·96	—3·8
150	139			Aur. †	7·8	23		45	30·0	58·29*	31	37	26·0	2·01*	8·3
152	49	M. 258		Gem.	7·8	23		50	27·9	51·86*	16	20	51·7	·04*	—7·0
153				——	7·8	23		50	30·7	52·08*	16	54	21·4	·04*	—6·3
157				——	7·8	24	96	4	36·0	51·96*	16	35	35·7	·12*	—6·5
158	50	23		——	8	24		6	45·0	52·10	16	56	43·0	·14*	—6·2
165	52			——	7·8	25		17	42·0	55·19*	24	44	33·4	·20*	1·5
167	243	53		Aur.	7·8	26		25	24·0	57·06	29	8	26·7	·24	5·8
168				Gem.	7·8	26		30	53·5	55·13*	24	36	31·0	·28*	1·4
169	55	24	γ	—— †	3	26		32	16·9	51·94	16	33	24·6	·29	—6·8
173	245	54		Aur.	6	27		43	59·5	56·75	28	25	29·5	·42	5·1
181	58	M. 260		Gem.	8	28	97	3	13·8	53·19*	19	49	39·4	·46*	—3·5
186	59	25		——	7	29		10	59·1	56·67	28	22	0·0	·50	5·0
202	62	26	(u)	——	5·6	31		41	19·5	52·42	17	49	38·8	·73	—5·4
204	64	27	ε	—— †	3	32		54	16·3	55·41	25	18	54·0	·74	2·0
207	65	28		——	6	32	98	1	6·0	57·03	29	9	31·4	·79	5·9
211	68	30	ξ. 1	—— †	5·6	33		10	28·5	50·64	13	25	0·5	·91	—9·8
217	71	31	ξ. 2	—— †	4	34		30	55·2	50·58	13	5	56·5	3·11	—10·1
240	80	33	(G)	——	6	38	99	34	34·5	51·83*	16	25	1·0	·34*	—6·8
243	82	35		——	6	39		46	56·4	50·80*	13	37	48·9	·41*	—9·5
247	84	36	d	——	6·7	40		53	19·5	54·06	21	59	1·0	·44*	—1·2
254				——	8	41	100	11	49·5	55·44*	25	32	29·0	·55*	2·4
264	94	37		——	6	43		45	0·0	55·32	25	36	43·0	·69	2·5
265				——	8·9	43		45	15·6	52·38*	17	55	15·5	·74*	—5·1
266	96	38	e (1)	—— †	5·6	43		50	21·3	50·74	13	25	9·4	·82	—9·7
270	98	M. 270		——	7	45	101	9	32·2	52·40*	17	58	58·4	·88*	—5·1
281	100	M. 271		——	7	46		30	54·0	52·46*	18	9	9·5	4·00*	—4·9
283	102	39	(y.1)	—— †	6·7	46		36	42·0	55·43	26	19	47·5	3·93	3·3
288	103	40	(y.2)	——	6·7	47		46	34·8	55·62	26	10	18·5	4·06	3·1
294				—— †	7	48	102	5	13·5	51·71*	16	12	9·0	·20*	—6·7
295				——	8	49		8	0·7	57·09*	29	28	53·0	·22*	6·6
296	105	M. 274		——	8	49		8	5·7	54·61*	23	42	15·2	·22*	0·7
297	106	41		—— †	6·7	49		11	25·0	51·72	16	20	33·4	·14	—6·6
302	108	42	ω.1	——	6	50		33	13·5	54·92	24	29	13·5	·32	1·5
305	109			—— †	6·7	51		41	43·5	57·14*	29	39	25·4	·41*	6·8
312	112	43	ζ	—— †	4	52	103	3	33·9	53·49	20	51	0·0	·57	—2·1
317	115	44	ω.2	——	6·7	53		18	46·8	54·21	22	55	23·8	·62	0·0
322	116	M. 279		——	8	55		39	54·4	52·36*	18	2	10·7	·74*	—4·8
329				—— †	9	57	104	8	28·5	51·53*	15	50	23·6	·90*	—7·0
330				—— †	7·8	57		8	24·0	57·44*	30	27	6·6	·90*	7·7
332				——	8·9	57		11	25·0	51·53*	15	49	58·4	·92*	—7·0
333	119	45	o	—— †	6	57		13	20·1	51·67	16	14	15·0	·99	—6·6
341	121	46	τ	——	5	58		35	52·8	57·43	30	33	32·2	5·05	7·7
343	122	47		——	6	59		44	29·1	55·84	27	10	16·5	·14	4·4
346	123			——	7·8	60		57	47·2	51·44*	15	38	57·0	·18*	—7·0
						VII hours.									
3	124	48	m	Gem. †	6	0	105	4	5·4	54·79	24	26	57·0	·21	1·7
5	125	49		——	8	0		7	37·2	55·43	26	4	8·8	·23*	3·3
				—— †	7·8	1		12·8			21	40·5			—1·0

Synonyms. P.	B.	F C M.	Character.	Constellation.	Mag.	VII hours. m.	Right Asc. °	′	″	A.V.+	Declination. + °	′	″	A.V.−	Lat. °
9				Gem.	8	1	105	18	6·0	51·71*	16	24	24·7	5·28*	—6·3
11	126	50		—— †	7·8	1		20	50·2	51·37*	15	30	3·3	·31*	—7·2
17	127	51	(w)	—— †	5	2		28	9·9	51·81	16	29	8·3	·33	—6·2
21	129	52	n	——	7	2		36	48·0	55·15	25	13	7·3	·40*	2·5
25	132	53	(z)	——	6	3		51	40·0	56·28	28	13	53·9	·44	5·5
35	134	M. 285		——	8	5	106	9	47·7	55·83*	27	2	9·6	·58*	4·4
39	136	M. 286		——	7	5		18	55·0	51·71*	16	29	8·8	·63*	—6·1
42				——	8	6		24	58·5	51·76*	16	38	5·1	·66*	—5·9
50	138	54	λ	—— †	4·5	7		38	51 9	51·89	16	53	19·0	·75	—5·7
57	139	55	δ	—— †	3·4	8	107	2	27·6	53·92	22	20	14·3	·88	—0·2
69	140	56	q	——	5·6	10		32	1·0	53·31	20	48	29·0	6·03	—1·7
75	141	57	a	——	6	11		49	1·5	55·09	25	25	17·4	·12	2·9
76	142	58		—— †	7	11		51	28·5	54·24*	23	19	0·4	·21	0·8
77	143	M. 292		—— †	7·8	11		51	36·9	52·45*	18	38	40·0	·14·	—3·8
83	144	59		—— †	6·7	12	108	1	26·2	56·15	28	0	36·9	·18	5·5
84				——	9	12		5	44·1	54·21*	23	18	18·0	·22*	0·9
89	146			——	8	13		18	42·3	58·03*	32	16	33·1	·29*	9·9
90	147	60	ι	——	4	13		19	18·0	56·15	28	10	56·4	·33	5·7
				—— †	8	14		26·5			20	52·6			—1·5
97	149	M. 294		——	7·8	15		45	1·5	53·65*	21	55	21·4	·44·	—0·4
98	150	61	r	——	7·8	15		47	6·8	53·22	20	38	38·5	·48	—1·7
				—— †	7	16		57			22	32			0·2
101	151	63	p	—— †	6	16		57	49·8	53·59	21	50	28·5	·59	—0·5
				—— †	8	16	109	0·5			15	42·4			—6·4
105	154	62	ϱ	—— †	5	16		3	22·8	58·01	32	10	4·8	·33	9·8
107	156	64	b. 1	——	5·6	17		12	53·7	56·18	28	30	59·6	·65	6·2
111	158	65	b. 2	——	5·6	17		20	15·9	56·14	28	18	56·5	·59	6·0
114				——	8	18		33	4·5	56·19*	28	18	46·1	·70*	6·0
117	17	6	ο	Ca. mi.	5·6	19		39	45·0	50·08	12	24	30·5	·71	—9·7
118				Gem.	8·9	19		44	54·7	56·05*	28	1	40·2	·77*	5·7
	162			—— †	6	20	110	3	55	52·1*	17	30	4	·9*	—4·5
128	165	66	α	—— †	2	22		27	13·0	57·67	32	18	45·0	7·06	10·1
129	166	67		——	7	22		29	54·9	51·41*	16	3	22·7	6·94	—6·0
131	167	68	k	——	5	22		32	45·0	51·48*	16	14	42·3	·96	—5·8
	168			—— †	7	22		36	16·	57·46*	31	22	53·7	7·05*	9·3
136				——	8	23		43	15·0	56·42*	29	3	2·0	·09*	6·9
138	169	69	υ	——	5	24		53	42·7	55·76	27	19	39·2	·19	5·2
144	171			——	7	25	111	19	45·0	53·03*	20	35	42·2	·29*	—1·3
146	174	M. 302		——	7·8	26		27	37·9	52·57*	19	21	22·2	·33*	—2·6
153	176	M. 303		—— †	7·8	26		31	46·5	54·62*	24	47	45·1	·35*	2·8
161	181	M. 304		—— †	7·8	27		46	30·0	54·56*	24	39	45·6	·42*	2·7
166	185	74	f	——	6	28		58	43 9	52·16	18	7	3·6	·45	—3·8
176	188			——	7·8	31	112	39	34·0	50·60*	13	56	10·6	·72*	—7·7
178	190	75	σ	—— †	6	31		41-	51·0	56·47	29	21	16·0	·95	7·4
179	192	M. 309		——	7	31		51	43·5	53·79*	22	51	30·3	·78*	1·0
182				——	8	32		58	21·9	55·50*	24	42	22·7	·82*	2·9
183	193	76	c	—— †	6	32		58	26·2	55·12	26	14	53·0	·79	4·4
184	194	77	κ	——	4	32	113	5	15·0	54·42	24	51	52·7	·91	3·1
191	195	78	β	—— †	2	33		15	49·9	55·26	28	29	46·8	·97	6·7
192	196	79		——	7	33		21	3·7	53·00*	20	47	1·4	·95*	—1·0

Synonyms. P.	B.	F.C.M.	Character.	Constel-lation.	Mag.	VII hours. Right Asc. m.	° ′ ″	A.V. +	Declination. + ° ′ ″	A.V. −	Lat. °
194	197	81	g	Gem.	6	35	113 37 55·5	52·22	18 59 9·0	8·05	−2·7
				—— †	8·9	36	52·5		29 8·7		7·5
				—— †	8·9	36	59·9		29 14·7		7·6
207	200	82	(B)	——	7	37	114 8 41·7	53·92	23 37 23·6	·18	2·0
	201			—— †	7	37	11 30	52·3*	18 49 22	·2*	−2·8
224	209	M. 314		——	7	40	115 4 28·0	52·56*	19 49 30·7	·49*	−1·6
232	210	84		—— †	7·8	41	16 51·0	53·68	22 50 14·3	·47	1·4
233	211	83	φ	——	5	41	18 30·0	55·28	27 16 13·2	·62	5·8
246	213	85	l	——	6·7	44	59 40·0	52·71	20 24 0·4	·78	−0·9
255	1	1		Can.	6	46	116 24 16·2	51·25	16 18 48·0	·88	−4·9
261	2			—— †	7	47	46 28·5	51·50*	17 2 44·0	9·03*	−4·0
267	3			——	7	48	117 6 3·7	50·39*	13 46 29·6	·13*	−7·2
270	4	2	α. 1	—— †	6	49	12 5·1	54·62	25 55 40·4	·14	4·7
272	217			Gem.	7·8	49	16 58·3	52·62*	20 21 9·2	·19*	−0·8
273				Can.	7·8	49	17 3·0	52·07*	18 46 52·6	·20	−2·1
275	6	3		—— †	6	49	19 40·5	51·74*	17 50 43·6	·20*	−3·2
276	8	4	ω. 2	—— †	6·7	50	24 38·7	54·55*	25 37 39·7	·18	4·5
279	10	5	(r)	——	6	50	31 18·6	51·37	16 59 43·5	·21	−4·0
280				——	8	50	36 17·7	52·58*	20 16 47·5	·29*	−0·8
285	219	6	χ	Gem. Can. }	5·6	51	48 7·5	55·53	28 20 33·4	·41	7·2
286				——	7·8	51	51 9·0	50·93*	15 29 37·7	·36*	−5·3
290	15	7		——	7·8	52	118 0 12·1	53·26	22 37 10·5	·39	1·6
295				——	8	54	28 6·0	51·85*	18 10 38·5	·55*	−2·6
296	18	8		——	6	54	28 48·1	50·29	13 40 39·5	·52	−7·1
297	19	M. 320		——	7·8	54	35 28·5	50·42*	14 3 43·0	·59*	−6·7
298	20	9	μ. 1	——	6	54	36 33·0	53·52	23 11 42·5	·57	2·3
299				——	7·8	55	41 15·0	53·48*	23 1 10·0	·62*	2·2
304	22	10	μ. 2	——	6·7	56	59 36·7	53·18	22 9 3·7	·75	1·3
307	24	11		—— †	7	57	119 8 36·0	55·40	28 3 1·2	·78	7·1
310	25	12	(s)	——	6	57	22 45·0	50·46	14 12 44·7	·81	−6·4
312	26	13	ψ. 1	——	7·8	58	31 30·0	54·50	26 25 16·0	·89	5·6
313				——	9	58	31 36·6	49·43*	11 4 57·1	·88*	−9·4
314	27	14	ψ. 2	—— †	7·8	58	35 45·0	54·50	26 6 7·5	10·26	5·3
317	28	M. 324		——	7·8	59	38 45·0	51·54*	17 35 31·5	9·91*	−3·0
				—— †	7	60	55 51		15 12 38		−5·3
						VIII hours.					
3	31	M. 328		Can. †	7	0	120 5 21·0	49·20*	10 24 14·2	10·05*	−9·8
4	33	15	ψ. 3	—— †	6	1	10 46·2	56·06	30 14 37·0	·11	9·4
5	32	16	ζ	—— †	6	1	10 51·6	51·83	18 14 21·8	·16	−2·3
				—— †	7·8	1	17 32		14 35 28		−5·7
13				——	7·8	3	38 45·4	49·50*	11 26 30·0	·22*	−8·7
14	37	M. 329		—— †	7	3	40 48·1	51·71*	18 16 3·8	·23*	−2·2
				—— †	7·8	3	48 15		13 38·6		−6·6
20				——	8	4	121 3 3·3	51·65*	18 10 17·3	·34*	−2·1
24	39			——	8·9	5	17 54·0	54·98*	27 39 14·3	·41*	7·3
26				——	8	5	20 37·3	49·03*	10 0 35·7	·43*	−10·0
37	47	18	χ	—— †	6	8	58 17·2	54·99	27 51 13·0	·98	7·5
41	50	19	λ	——	6	9	122 9 17·7	53·73	24 38 26·7	·71	4·4
42	49			—— †	6·7	9	9 54·7	52·64*	21 22 3·0	·67*	1·3
48				——	8	11	51 31·8	49·37*	11 17 20·1	·87*	−8·5
50	54	20	d. 1	——	6	12	58 28·6	51·84	18 57 47·4	·93	−1·0

Synonyms.			Character.	Constellation.	Mag.	VIII hours. Right Asc.					Declination. +				Lat.
P.	B.	F.C.M.				m.	°	'	"	A.V.+	°	'	"	A.V.−	°
51	55			Can.	8	12	122	59	20·7	51·73*	18	45	59·6	10·91*	−1·2
53	56	21	(f)	—	7	13	123	14	30·0	49·30	11	16	0·8	·96	−8·5
54				—	7·8	13		20	23·4	51·40*	17	49	27·6	11·02*	−2·0
59	58	22	φ. 1	— †	6·7	14		34	1·8	54·99	28	32	26·3	·20	8·4
61				— †	9	14		37	11·7	55·15*	28	42	14·5	·10*	8·6
62	59	25	d. 2	— †	6	14		37	21·0	51·09	17	41	39·7	·25	−2·1
64	60	23	φ. 2	— †	6	15		39	58·5	54·66	27	34	36·0	·13	7·5
65	62	24	υ. 1	— †	7	15		41	12·0	53·90	25	10	49·0	·23	5·2
68	67	27		—	6·7	16		54	50·4	49·92	13	18	15·0	·29	−2·4
76	70	28	υ. 2	—	6·7	17	124	10	55·0	53·55	24	47	51·4	·33	4·9
77	71	29		— †	6	17		21	40·5	50·36	14	51	47·3	·25	−4·8
79	72	M.341		— †	8	18		26	33·9	53·72*	25	0	0·0	·34*	5·2
80	73	M.340		—	7·8	18		26	51·1	54·37*	26	50	50·5	·38*	7·0
84	75	30	υ. 3	—	6·7	20		54	52·5	53·50	24	44	42·0	·51	5·0
85	76	31	ϑ	— †	5·6	20	125	2	31·8	51·45	18	45	36·5	·55	−0·8
86	77	M.344		—	7·8	20		2	57·7	51·87*	19	39	5·0	·53*	0·1
88	80	33	η	—	6	21		16	49·2	52·29	21	6	35·7	·64	3·6
89	81	32	υ. 4	—	7·8	21		17	20·1	53·45	24	45	14·5	·63	5·1
91	83	34	(h)	—	6·7	22		26	28·5	49·09	10	44	3·5	·60	−8·5
98	84	M.347		—	7·8	23		39	49·5	50·05*	13	55	53·0	·73*	−5·3
101	87	35		—	8	24		57	6·4	51·96	20	16	2·5	·75	0·9
104	88	M.349		—	8	24	126	3	27·7	52·06*	20	27	0·5	·81*	1·1
106	90	M.350		—	7·8	25		13	25·0	50·65*	15	59	44·5	·85*	−3·2
111	93	36	c. 1	—	7	26		33	35·2	48·85	10	20	27·0	·98	−8·6
112	94	M.351		—	8	26		34	27·0	51·86*	19	57	11·7	·99*	0·7
116	95	37	c. 2	—	7·8	27		48	35·7	48·84	10	15	50·0	·98	−8·6
118				— p	8	28		53	49 5	51·97*	20	22	7·6	12·04*	1·2
119				— p	8·9	28		54	39·0	51·94*	20	17	5·6	·06*	1·1
121				— p	8	28	127	2	37·8	52·09*	20	46	49·5	·07*	1·6
122	96	38	o	— p	7	28		2	59·1	51·98*	20	28	20·7	·08*	1·3
124	98	M.355		— p†	7	28		5	11·1	51·91*	20	14	8·4	·09*	1·1
126	100	39		— p†	6	29		8	44·1	52·05*	20	42	12·7	·10*	1·5
127	101	40		— p†	6	29		10	3·6	52·04*	20	40	1·4	·11*	1·5
128				— p	8	29		12	3·7	51·90*	20	13	43·7	·12*	1·1
129	102	M.359		— p	7	29		12	58·5	51·94*	20	21	58·7	·12*	1·3
130	103	41	ε	— p	6·7	29		14	24·0	51·90*	20	14	30·4	·13*	1·1
132	105	42	e	— p	7·8	29		18	10·9	51·95*	20	25	1·7	·15*	1·3
134	107	M.362		— p	7·8	29		21	42·4	51·90*	20	16	45·5	·17*	1·2
135		M.357?		— †	8	30		25	24·0	52·18*	21	10	33·7	·18*	2·1
136	110	M.362		— p	7	30		34	52·5	51·97*	20	34	34·6	·23*	1·5
142	113	43	γ	— †	5	32		55	19·5	52·30	22	10	39·0	·25	3·2
143	114	44		—	8	32		56	12·0	51·41*	18	51	25·8	·32*	−0·1
144	116	45	a. 1	—	6·7	32	128	2	30·0	49·73	13	23	18·2	·32	−5·3
150	119	47	δ	— †	4·5	33		19	27·4	51·42	18	52	46·5	·66	0·1
154	120	49	b	—	6·7	34		28	9·7	49·00*	10	47	46·4	·47*	−7·7
156	121			—	7·8	34		31	22·5	51·58*	19	31	58·3	·48*	0·6
161				—	7·8	36		52	48·0	49·13*	11	18	52·8	·58*	−7·0
163	131	50	a. 2	—	6	36		59	17·5	49·39	12	50	7·0	·61	−5·6
170	133	M.370		—	7·8	38	129	24	42·4	49·67*	13	16	30·0	·73*	−5·1
171				—	8·9	38		25	55·5	49·68*	13	19	27·4	·73*	−5·0

Synonyms.			Character.	Constel-lation.	Mag.	VIII hours. Right Asc.					Declination. +				Lat.
P.	B.	F.C.M.				m.	°	'	"	A.V. +	°	'	"	A.V. —	°
179	138	M.371		Can.	7	39	129	49	49·5	51·24*	18	44	14·5	12·84*	0·3
180	139	M.372		—	7	39		50	16·9	51·49*	19	34	5·5	·84*	1·1
181				—	8·9	40		53	58·5	51·24*	18	46	7·8	·86*	0·3
182	140	54		— †	6·7	40		57	58·5	50·20	16	5	2·8	·68	—2·2
183	141	52	(m)	—	7·8	40		59	28·2	50·57	16	44	9·1	·75	—1·6
191	150	M.374		— †	7	42	130	28	38·2	50·99*	18	6	53·5	13·01*	—0·2
195	153	M.375		— †	7·8	42		36	58·5	51·77*	20	42	49·2	·05*	2·4
196	155	M.376		— †	7·8	43		38	4·5	51·04*	18	17	31·7	·06*	0·1
197	156	M.377		—	8·9	43		39	10·0	50·13*	15	9	20·3	·06*	—3·0
200		M.379 ?		Hyd. †	8·9	43		48	13·5	48·46*	9	10	5·5	·10*	—8·8
203	157	M.378		Can. †	8	43		50	21·9	50·07*	14	59	33·2	·11*	—3·1
206	160	M.380		— †	7	44	131	1	26·4	50·92*	17	58	59·0	·16*	—0·1
208	162	M.381		— †	8	45		8	21·0	50·04*	14	56	9·8	·19*	—3·1
211	164	60	α. 1	—	6	45		14	50·4	49·31	12	22	52·3	·23	—5·5
213	166	M.385		— †	8	45		21	55·8	50·97*	18	14	20·3	·25*	0·2
217	169	M.383		—	7·8	46		28	5·4	50·87*	17	54	13·5	·28*	—0·1
218	170	62	o. 1	—	6	46		31	6·3	50·34	16	4	47·8	·28	—1·9
219	171	63	o. 2	—	6	46		35	56·1	50·34	16	20	20·8	·31	—1·6
	172			— †	6	47		43	29	48·7*	10	8	54	·3*	—7·5
222	175	65	α. 2	— †	5	48		52	59·4	49·37	12	37	22·0	·43	—5·1
224	176	M.388		—	7	48		57	54·7	51·12*	18	54	16·0	·41*	1·0
225	177	M.389		—	8	48	132	6	27·0	49·68*	13	50	32·8	·44*	—3·9
231	181	68	(p)	— †	7·8	50		37	13·5	50·68	17	51	22·8	·57*	0·1
234	183	69	υ	—	6	51		45	15·6	52·90	25	13	49·9	·59	7·3
240				—	8·9	52	133	3	19·5	49·93*	14	57	55·5	·69*	—2·5
				— †	8	53		17·0			25	23·8			7·4
244	189	M.391		—	7·8	54		25	57·0	49·01*	11	38	12·5	·71*	—5·6
248	191	71		— †	7·8	54		37	43·0	50·78*	18	10	43·5	·83*	0·7
250		73 ?		— †	8	55		48	36·7	50·18	16	3	57·5	·88*	—1·2
252				—	8	56		54	39·0	50·16*	16	0	39·7	·89*	—1·2
	194			— †	6	56		58	34	52·4*	23	46	39	·9*	6·2
253	200	76	κ	—	5·6	57	134	13	37·5	49·11	11	27	50·4	14·00	—5·6
256	202	75		— †	6·7	57		14	50·2	53·26	27	26	50·3	·41	9·8
257		74?		— †	7·8	57		15	50·2	50·00*	15	30	32·2	13·99*	—1·9
258	206	78		— †	7	58		27	2·1	50·60	18	16	17·5	·97	1·0
259	205	77	ξ	—	5·6	58		27	33·0	52·04	22	50	44·7	·98	5·4
262	208	79		—	6	59		42	28·0	52·05	22	47	59·5	14·06	5·4
263	209	M.395?		— †	7·8	59		43	18·0	49·14*	12	22	12·5	·09*	—4·5

ADDITIONAL NOTES to the first and second Portions.

Anon. R. A. 23° 23'.) In Harding's Atlas are 2 stars of the ninth magnitude.

P. I. 174.) Upon an inspection of Harding's Atlas, it may be doubted whether Herschel has not mistaken 4 Arietis for 3; in which case his double star V. 92 will be placed in R. A. 24° 32', and Decl. 15° 20', or thereabout.

P. II. 96.) This is No. 12 of Herschel's new Cat. of double stars,

stars, printed in the Memoirs of the Astronomical Society. This will hereafter be quoted by the abbreviation "H. n. C."

B. 34 Tauri.) See H. n. C. 23.

55 Tauri.) Flamsteed's Declination requires $-8'$. In Herschel's Index to Flamsteed, and in Phil. Trans. 1799, the correction is given with a wrong sign.

P. V. 43.) In the note, for "is left out of," read, "has $-1°$ of Decl. in."

P. V. 225.) The place of Herschel's star I. 67 is probably R.A. 84° 12′. Decl. 32° 50′.

At R.A. 89° 18′.—Decl. 14° 1′, is a cluster of stars, Hers. VIII. 24: in which is a double star, I. 57. Preceding 70 and 67 Orionis. "A spot which appears nebulous in the finder, and is about 50′ from 67 Ori. and 45′ from 70. More than 12 stars in view with 460; among them is a double star. The largest of the base of an isosceles triangle, n. preceded by four stars in a line. Considerably unequal. With 460, one full diameter of L. Position 19°·8 s. following."

NOTES to the third Portion.

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68 Orionis.) The precession in Decl. is $-0''·015$, the proper motion $+0''·05$. Near this is a double star. Hers. VI. 72. "The most N. of two that are one degree asunder. Very unequal. L. w; S. dr. Distance with 278, 72″·83. Position 41°·0 s. preceding.

6 Geminorum.) The prec. in Decl. $-0''·016$; pr. mot. $+0''·03$.

69 f. 1 Orionis.) The prec. in Decl. $-0''·045$; pr. mot. $+0''·06$.

P. VI. 13.) Double. The following star, 14 of Pi. 8 mag. R.A. $+9''·0$. Decl. $-1'56''·6$.

44 $\varkappa$ Aurigæ.) Pr. motion in R.A. $-0''·14$; in Decl. $-0''·32$. This latter is confirmed by an obs. of the Decl. by Flamsteed.

7 η Geminorum.) Called *Tejat.*

Anon. R.A. 90° 44′.) Double. Observed by Lalande. *Hist. Cel.* 313.

72 f. 2 Orionis.) See Hers. V. 23. "A double star following f." Distance about 40″.

9 Geminorum.) Mayer's declination requires $+2'$.

12 ————.) Hers. V. 55. "A small star *near the place of* 12 Gem. Treble. The two nearest a little unequal. Distance less than 1′.

13 μ Geminorum.) Called *Tejat* posterior.

15 Geminorum.) Double. The preceding star, Pi. 99. mag. 8. R.A.

R.A. $-16''\cdot3$. Decl. $-32''\cdot6$. Hers. V. 56. "Considerably or very unequal. L. r; S. d. Distance, $32''\cdot65$. Pos. near 60° s. preceding." See also V. 52, which appears to refer to the same star. "Double, the 2nd star from ν towards μ Gem. Pretty unequal. L. r; S. b. Distance 35′, inaccurate."

Anon. R.A. 94° 3′.) This may be the star whose brightness Sir W. Herschel estimated as 17 Gem. The place of that star in Fl. is 1^m following 15 Gem. in which spot no star exists. It turns out that Fl. has only one obs. which could be referred to 17 Gem. and in that the time is marked doubtful, so that most probably the obs. belongs to 15.

P. VI. 120.) Double. H. n. C. 111. "About 25′ or 30′ n. f. 18 ν Gem. A very small star, 5th class. L. r; S. d, very unequal, or rather, extremely unequal. Pos. $77°\cdot2$ s. f.

Anon. R.A. 94° 44′.) A star 1° 40′ from ν Geminorum. Supposed to be identical with H. n. C. 141. "Double, 2nd class. It is 1° 20′ n. f. 18 Gem. in a line parallel to γ and ϵ. Equal, or the preceding perhaps the smallest." Position from *Hist. Cel.* 272.

20 Geminorum.) Piazzi calls this 21 Gem. and the star preceding it (134) he calls 20 Gem. There is however reason to suppose that Flamsteed never observed the star as double, and that his 21 arose from an obs. of 20 as a single star, but with an error of 1^m in R.A. Pi. 134. mag. 8. R.A. $-14''\cdot4$. Decl. $-18''\cdot6$, whence the distance $23''\cdot1$ and position $53°\cdot6$ s. p. Herschel's description is as follows, "21 : : Geminorum. Double; a little unequal. Both pr. Distance about 25″." He observes that 20 and 21 are not in the heavens as they are marked in Fl. Atlas.

Anon. R. A. 95° 30′) Double. Lal. *H. C.* 272, foll. star, 8m. R.A. $+4^s\cdot7$. Decl. $+24''$. Hers. V. 112. "Forms almost an isosceles triangle with μ and ν Gem. Nearly equal. The preceding p r, the following w r. Distance fifth class far.

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P. VI. 144. or *B.* 48 Gem.) The R. Asc., as given by Bode from Lalande, is $+10'$.

P. VI. 150. or *B.* 139 Aur.) The declination, as given by Bode from Lalande, is $+10'$.

24 γ Geminorum.) Called *Alhena.* Hereabouts, two or more double stars. Hers. IV. 28. "Double. Near γ Gem. towards ζ Tauri. A little unequal. Both r. Distance $19''\cdot7$. Pos. $57°\cdot0$ s. prec." Also, V. 71. "Double, 3′ or 4′ n. prec. γ Gem. Of the 5th class. More in view." And again, VI. 91. "Double, 3′ or 4′ n. of γ Gem. Considerably unequal. Both small; too obscure for measures with

with 7-feet; my 20-feet shows a third star between them, with 12 inches aperture." The two last mentioned were observed on the same day.

27 ε Geminorum.) Called *Mebsuta.* Double. Hers. VI. 73. "L. w. Distance 110″·5."

30 and 31 ξ Gem.) The magnitudes of these stars are variously set down in Flamsteed and Mayer.

38 e Geminorum.) Double. Hers. III. 47. "Extremely unequal. L. rw.; S. r. Distance, with 460, 7″·8. Position 89°·9 s. foll. Two more in view, the nearest of them perhaps 40″; they form a rectangle nearly."

39 Geminorum.) The proper motion is deduced from Br. and Pi., viz. R.A. −0″·30. Decl. +0″·11 per annum.

P. VI. 294.) Pi. calls this 41 Geminorum, and he suspects a proper motion. But see the next following note.

P. VI. 297.) This is 275 of Mayer's Zod. Cat. and according to Bessel is the true 41st of Flamsteed.

P. VI. 305, or *B.* 109 Gem.) Lalande's Decl., as given by Bode, is −10′.

43 ζ Geminorum.) Called *Mekbuda.* Bode estimated it scarcely so bright as the fourth mag. in 1801, which accords with Piazzi. In the older catalogues it is set down as 3·4 or 3 m. Herschel's comparative estimate will be found in the note to λ Gem. It has a star 8·9 mag. preceding. Pi. 311. R.A. −8″·7. Decl. +88″·0. Hers. double stars VI. 9. "Very unequal. L. rw. S. dr. Distance 91″·87, rather full measure. Pos. 81°·23 n. preceding."

P. VI. 329, 330.) Either there is an error in the R.A. of one of these stars, or they are not placed in their proper order.

45 o Geminorum.) A star 7 mag. precedes this about 3^s, north 6′. *Piazzi.*

48m ————.) Burckhardt (in *Conn. d. T.* 1820) supposes that *C. H.* 159 is the same star with this, only with an error of 2° of declination. There is a star of 8th mag. in Harding's Atlas, about R.A. 104° 58′. Decl. 22° 42′, but it is not in Lalande's *Hist. Cel.*

Anon. R.A. 105° 13′.) From Lalande *H. C.* p. 272. Double, H. n. C. 94. "South preceding δ Gem. near 2° in a line parallel to 60 and 27 ε Gem. A third star near. About the 4th class."

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50 Geminorum.) Herschel in Phil. Trans. 1797, says that Flamsteed never observed 50 Gem., and that the star of which he there gives the brightness is at a considerable distance from the place assigned by the Brit. Cat. And yet the place of *P.* VII. 11 differs but little from Fl. 50

(R.A.

(R.A. 105° 12′ 15″. Decl. 15° 28′ 58″.) The remainder of Herschel's remarks are unintelligible.

C. H. 139, set down as preceding 51 Gem. 25′ 22″ and *north* 49′ 35″, is not to be found in Bode, Piazzi, Lalande, or Harding; but if we substitute *south* for north, the place will then agree with *P.* VI. 346.

51 Geminorum.) Hers. VI. 74. "Has two very obscure stars in view. L. r. S. r. S. r. The nearest about 1½′, the next 2′. Pos. of both about 40° or 50° n. following.

54 λ Geminorum.) Is marked 5 mag. in the Brit. Catalogue. Bode supposes it to be changeable, and estimates it of the 3rd mag. in 1801. Herschel (Phil. Tr. 1796) gives its lustre thus, λ; δ. κ, ϑ—ζ. An interval of 9 months between two observations seemed to indicate an increase of brightness.

55 δ Geminorum.) Called *Wasat.* Double, Hers. II. 27. "Extremely unequal. L. w, inclining to r; S. r. With 227, about 2½ full diameters of L; with 460, 4 or 5 diam. Position 85°·85 s. prec."

58 Geminorum.) Two obs. of Bradley, compared with Pi. indicate a pr. motion in R.A. of −0″·15.

M. 292.) Mayer's observation of this star is imperfect.

P. VII. 83.) A star, 7·8 mag. about 30ˢ preceding, 5′ north.

Anon. R.A. 108° 26′.) From Lalande, p. 272. Double, foll. star 9 m. R.A. +0ˢ·3. Decl. +6″. Hers. III. 48. "About ½° n. prec. 61 r Gem. in a line parallel to κ and 60; near 2° from δ. A little unequal. Both pr. Distance 6″·25. Pos. 43°·9 n. foll." This must surely be identical with H. n. C. 95. "South foll. δ Gem. towards r, about 25′ from r; third class, a little unequal."

Anon. R.A. 108° 57′.) A star in Harding, but not in the *Hist. Cel.* or any Catalogue. Double, Hers. V. 66. "About ¾° n. of, and a little preceding 63 p Gem. in a line parallel to υ and α. Very unequal. L. pr; S. d. Distance 34″·65. Pos. 1° or 2° n. preceding."

63 p Geminorum.) Double, Hers. V. 53. "The brightest of two. Extremely unequal. L. pr; S. d. Distance 44″·25."

Anon. R.A. 109° 0′.) From Lalande *H. C.* 51. Double H. n. C. 108. "2° 40′ s. f. 54 λ Geminorum, towards β Cancri, 1st class, pretty unequal."

62 ρ Geminorum.) Pr. mot. in R.A. +0″·08 in Decl. +0″·22. The star is called s in Flamsteed's and most subsequent catalogues; but this, according to Bessel, is owing to a typographical error in the former work, and copied into the others.

66 α Geminorum.) The proper motion of *Castor*, according to

Br.

Br. and Pi. is in R.A. $-0''\cdot22$, in Decl. $-0''\cdot05$. A very remarkable double star. According to Piazzi the preceding star is 3·4 mag., the following 3 mag., diff. R.A. $5''\cdot8$ determined with the utmost care: diff. Decl. $0''\cdot0$. In a note he remarks, that Dr. Hornsby first determined the distance of the stars in R.A. $=3''\cdot8$, which measure remained constant for 20 years. Castor is the first of Sir W. Herschel's 2nd class of double stars, and in his original catalogue (Phil. Tr. 1782) he sets down the distance $5''\cdot156$ diameters included. Pos. 32°·8 n. prec. In the volumes for 1803 and 1804, this indefatigable observer stated the results deduced from a series of observations from 1783 to 1803, viz.—that the two stars revolved round their common centre of gravity in 342 years; in a plane nearly perpendicular to the visual line. This discovery, which appears to have been wholly unknown to Piazzi, will account for the discordance between his measurement and that of Hornsby.

B. 162 Geminorum.) This is the star, situate between 68 and 61 Gem. noticed by Hers. (Phil. Tr. 1783) as among the considerable stars not comprised in any existing catalogue.

B. 168 Geminorum.) Position from Bradley.

M. 303, 304.) The synonyms are wanting in Piazzi.

75 σ Geminorum.) Pr. mot. R.A. $+0''\cdot06$. Decl. $-0''\cdot21$.

76 c ——————.) Erroneously called L in Piazzi.

78 β ——————.) Called *Pollux*. The R.A. in the text is that given by Bouvard. (*Conn. d. T.* 1821.) That of Piazzi is $-0''\cdot3$. Both authorities agree in the Declination. The proper motions are as follow:

	R.A.	Decl.	Great Circle.
As given by Bouvard	$-0''\cdot647$	$+0''\cdot080$	......
From Br. and Piazzi	$-0''\cdot742$	$-0''\cdot058$	$0''\cdot655$
Piazzi's own comparison	$-0''\cdot72$	$-0''\cdot11$	......

A multiple star. Hers. VI. 42. "Extremely unequal. The nearest distance $116''\cdot75$, rather full measure. Pos. 24°·5 n. foll. not extremely accurate. This is the smallest. The next distance $197''\cdot3$ pretty accurate. Pos. 15°·93 n. following." The same observer, in Jan. 1796, records thus, "β Gem. appears to be of a deeper colour than it was a good many years ago. I should now place it among the red or ruddy stars, which formerly I did not use to do."

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Anon. R. A. 113° 52′.) From Lal. *H. C.* 53. Double. Hers. II. 65. "Full $\frac{3}{4}$° n. foll. β Gem. in a line from δ continued through it; the star next to the middle one of three, nearly in a line. Excessively unequal; L. r. w. S. d.

With 227, above $2\frac{1}{2}$ or near 3 diameters of L, and 5 other stars in view; with 460, above 3 diam. of L. Pos. 89°·2 n. foll."

Anon. R. A. 113° 59′.) From Lal. *H. C.* 53. Double. Hers. V. 67. " Near 1° n. foll. β Gem. nearly in a line from δ continued; the furthest and smallest of three. Considerably unequal. L. r. S. dr. Distance 47″·6."

B. 201 Geminorum.) From Lalande. Double. Hers. II. 64. " About $\frac{1}{2}$° s. foll. 81 g Gem. nearly in a line from ζ continued; the nearest and largest of two. Very unequal. L. r; S. blueish r. With 227, above 3 diameters of L. Position 4°·15 n. preceding."

84 Geminorum.) Is of the 7th mag. only.—*Bode.* Although marked 5 m. in the Brit. Cat., it has no magnitude in Fl. observations.

P. VII. 261, or *B.* 2 Cancri.) Is the same as *C. H.* 140.

2 ω. 1 Cancri.) Double. The small star 3″ north, very faint. —*Piazzi.*

3 Cancri.) Astronomers have been much puzzled to account for the erroneous declination given to this star in Bradley's Catalogue (Naut. Alm. 1773). An inspection however of Bessel's Cat. clearly shows that although Bradley's R. A. is correct, his Decl. belongs to a star about 56′ north, which is *P.* VII. 273. This latter star had not appeared in any earlier catalogue.

4 ω. 2 Cancri.) " Has a very obscure star in view. L. pr. Distance about $1\frac{1}{4}$ minute. Pos. about 30° n. prec. A third about 2′. Pos. more north." Hers. VI. 75.

11 Cancri.) Double. Hers. I. 11. " Considerably unequal. Both pale r. With 227, 1 full diam. of L. with 460, about $1\frac{3}{4}$ diam. Pos. 85°·17 n. prec."

14 ψ. 2 Cancri.) Pr. mot. R. A. −0″·05. Decl. −0″·36.

Anon. R. A. 119° 56′.) From Lal. *H. C.* 52, 279. Is 13 Cancri of Fl. Cat. edit. 1712, although omitted in the standard edit. of 1725. See *C. H.* 161.

16 ζ Cancri.) Double. Pi. foll. star VIII. 6. mag. 7·8. R. A. +2″·4. Decl. +6″·0. Hers. I. 24, and III. 19. " A most minute treble star. It will at first sight appear double only, but with proper attention, and under favourable circumstances, the preceding of them will be found to consist of 2 stars, which are considerably unequal." The single star is of intermediate magnitude between the other two, which latter " are both pale r. or r. With 278, but just separated, with 460, distance $\frac{1}{4}$ diam. of S. Pos. 86°·53 n. following." The single star " pale r. Distance 8″·046 mean measure. Pos. 88°·27 s. prec."

M. 328.

M. 328.) Is C. H. 165. Mayer's R. A. requires $-15'$.

15 ψ. 3 Cancri.) Ought not this properly to be ψ Geminorum? See Hevelius, and the original obs. of Flamsteed.

Anon. R. A. 120° 17′.) From Lal. *H. C.* 52, 279. Is *C. H.* 162.—Fl. 15 Cancri. ed. 1712.

M. 329.) Mayer's Decl. doubtful. Double. Hers. VI. 78. "About $\frac{1}{2}$° foll. ζ Cancri towards η Leonis. Extremely unequal. Dist. 63″·8." A telescopic star precedes (to the N.) about 10^s.—*Piazzi.*

Anon. R. A. 120° 48′.) From Lalande, 216. Is *C. H.* 163.

18 χ Cancri.) Pr. mot. in Decl. $-0''\cdot37$.

P. VIII. 42, or *B.* 49 Cancri.) Is *C. H.* 93, 5th magnitude. Lalande (*H. C.* 211) calls it "Etoile singulière."

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22 φ. 1 Cancri.) Pr. mot. R. A. $-0''\cdot11$. Decl. $-0''\cdot12$. Double. *H.* VI. 109. "Very unequal, L. r; S. dr."

P. VIII. 61.) This star is not far from the place of 26 Cancri, which was not observed by Fl. and in fact never existed.

25 d. 2 Cancri.) Pr. mot. R. A. $-0''\cdot25$. Decl. $-0''\cdot15$.

23 ϕ. 2 Cancri.) Double. Hers. II. 40. "A little unequal. Both rw. With 227, near 2 diameters, with 460, $2\frac{1}{2}$ diam. of L. Pos. 56°·7 n. following."

24 υ. 1 Cancri.) Double. Pi. foll. star. 66. mag. 7·8. R. A. $+3''\cdot9$. Decl. $+3''\cdot6$. Hers. II. 41. "Consid. unequal. Both pr. With 227, $1\frac{1}{2}$ diam. of L; with 460, 4 diam. Pos. 32°·15 n. following." Mayer's 338 should seem to be the same star, with an error of 20′ in declination.

29 Cancri.) In Piazzi's Cat. *for* Geminorum *read* Cancri.

M. 341.) Is *C. H.* 166.

31 ϑ Cancri.) Double. Hers. V. 59. "Extremely unequal. L. r. S. d. Distance 44″·53. Pos. n. foll.

P. VIII. 118.) The thirteen stars marked with a P, belong to the cluster called *Præsepe.*

M. 355.) The star *C. H.* 168 (called f in Fl. obs.) agrees with this within 10′ of declination.

39 Cancri.) Fl. declination requires $+5'$.

40 Cancri.) Fl. declination requires $-2\frac{1}{2}'$.

P. VIII. 135.) Mayer's 357 is an imperfect observation. Wollaston places it in R. A. $-17'\ 5''$. Decl. $-4'\ 22''$. but in Bode's Catalogue (109) the R. A. is $+52''$.

43 γ Cancri.) Called *Asellus Borealis.*

47 δ Cancri.) Called *Asellus Australis.* Pr. motion in R. A. $+0''\cdot03$, in Decl. $-0''\cdot23$.

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54 Cancri.) Pr. mot. R. A. $-0''\cdot24$. Decl. $+0''\cdot20$. Double. Hers.

Hers. IV. 111. "A little unequal. Both rw.; S. a little darker. Distance 17″·23. Pos. 29°·0 s. following."

P. VIII. 200.) Double. Pi. following star (201) R. A. +12″·0. Decl. −7″·5. Mag. 9. Mayer's obs. doubtful.

M. 374. 375. 376. The synonyms are omitted by Piazzi.

M. 378.) Mayer's obs. imperfect.—*Piazzi.*

M. 380.) "Etoile rouge."—*Lalande.*

M. 381.) Mayer's observation imperfect.

M. 385.) The place of this star in Bode's Cat. as given from Mayer, agrees nearly with that in the text, but in Wollaston the differences are, R. A. +11½′, and Decl. −1¼′.

B. 172 Cancri.) The author has taken the liberty to correct what he considers a misprint in Bode's Catalogue, by substituting 10° for 12°. This correction is sanctioned by Lalande's obs. and by Harding's Atlas. *C. H.* 360 appears to be the same star.

Anon. R. A. 133° 17′.) From Lalande, 148. It is *C. H.* 167.

71 Cancri.) The R. A. of the Brit. Cat. requires +10′. Herschel has remarked that the star is wrongly laid down in Fl. Atlas.

P. VIII. 250.) Piazzi calls this 73 Cancri, of which no obs. by Fl. is to be found. Bode and Herschel consider it as erroneously introduced into the Brit. Cat. where the R. A. is greater by 11′ than that of Piazzi's star.

B. 194 Cancri.) Double according to Bode, who settled its place. Neither he nor Mr. South (Mem. Astr. Soc.) seems to have recognised this to be Herschel's star III. 92. "About 1° n. prec. ξ Cancri, in a line parallel to ε Leonis and 40 Lyncis, a considerable star. A little unequal. Both rw. Dist. 8″·83. Pos. 65°·2 s. preceding."

75 Cancri.) Pr. motion. R. A. −0″·21. Decl. −0″·42.

P. VIII. 257.) The place of 74 Cancri is R. A. −12′ 2″. Decl. −10′ 32″, and no observation by Flamsteed is to be found.

78 Cancri.) Fl. R. A. requires −2½′.

P. VIII. 263.) The R. A. of Mayer 395 is incomplete, and if this be the star observed, it requires −23′. In Bode's Cat. Mayer is quoted as the authority, and yet the R. A. is right.

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† The author wishes for some information respecting a star marked *Variable* in Harding's Atlas; it is about 50′ preceding 31 Virginis. R. A. 187° 4′. Decl. +8° 5′. No such star is mentioned in any other work within his means of examination.

XII. *Ob-*

XII. *Observations on M.* LAPLACE's *Communication to the Royal Academy of Sciences*, "Sur l'Attraction des Sphères, et sur la Répulsion des Fluides élastiques." *By* JOHN HERAPATH, *Esq.*

ON the first of May 1821, I published in the Annals of Philosophy a theory of gaseous bodies, mathematically drawn from the Newtonian theory of heat. An announcement of the publication and objects of the paper which contained this theory, and which had been in the hands of the principal members of the Royal Society from the May preceding, was sent to the Marquis de Laplace in June 1821. On the 10th of the following September, this nobleman communicated to the Royal Academy of Sciences a paper, whose professed object is to demonstrate from the principles of caloric the known laws of permanent airs,—the same that my paper contained. Unfortunately I did not meet with M. Laplace's paper until towards the fall of 1822; at which time I first saw it in the *Connaissance des Tems* for 1824.

Though it was obvious from the perfect coincidence of the object of M. Laplace's paper with that of a part of mine, and its being presented to the Royal Academy so long after the printing and notice of my paper, that his communication was in consequence of mine and intended to supersede it, yet I preferred leaving some instances of arguments and results, which appeared to me in point of accuracy to be exceptionable, to the comments of others, to making any observations on them myself. Perceiving, however, by the *Connaissance des Tems* for 1825, which I have lately received, that M. Laplace has in that work as good as four papers in continuation of his first; and that he has excited such an interest in the French Board of Longitude, as to induce that body to issue a commission to repeat some experiments on sound, for the purpose of affording him the advantage of better results; I have thought it necessary to throw together a few remarks, which may enable philosophers more easily to estimate the success of M. Laplace's investigations.

In the views of corpuscular repulsion of airs, which Newton proposed to philosophers to examine, he imagined that the repulsion of each particle extends to those particles only which immediately surround it. The reason, if a reason it can be called, which I believe he assigns for this limitation, is the similarity of a phænomenon in magnetic attraction. Without entering into a discussion of the difference of those phænomena, which are as different and dissimilar as they can well be, it

it may be said that explanations by analogy are in most physical cases illusive and deceitful, and in all unsatisfactory. M. Laplace therefore, discarding the limitation of Newton, proceeds to determine the laws of elastic fluids on the supposition of the corpuscular repulsion being sensible at insensible, and insensible at sensible distances. Each particle of a fluid which is at a sensible distance from the envelope, is on this hypothesis kept in equilibrio by the balance of repulsion in the surrounding particles. This repulsion he first assumes exclusively due to the caloric of the particles; their mutual distances being such, though insensibly small, that their reciprocal attraction has no sensible effect. In the general equation therefore of a fluid sphere,

$$dp = \rho \phi \, dr,$$

in which ϕ is the repulsion of the whole sphere of the density ρ on a point at the distance r from its centre, and p the pressure in an opposite direction to the repulsion, M. Laplace conceives $\phi = o$; which gives $p =$ constant.

So far I apprehend no great objection would be made to M. Laplace's assumptions; though some of them are certainly not unexceptionable. His statement however, that "en nommant r la distance mutuelle de deux molécules de gaz, nous exprimerons la loi de répulsion par $\mathrm{H}\, c^2\, \phi\,(r)$," $\phi\,(r)$ being insensible with a sensible value to r, and H being a constant, we cannot I think so easily admit. For since the particles of caloric are supposed to have a mutually repulsive force, and each particle of the gas to retain by its attraction its caloric, the caloric must assume about a particle of the gas, the form of a sphere or spherical shell. Nor would the repulsion of the surrounding particles have any effect on the figure, unless to promote or preserve it. Supposing therefore the distance r between the particles to remain the same, the function $\phi\,(r)$ must involve the dimensions of the spheres or shells; consequently, as these dimensions would vary with the quantity of caloric, the repulsion would not be as c^2, as M. Laplace conceives; unless when the particles of caloric mutually repel one another by a force reciprocally proportional to the square of the distance, which would give the gas a very different law to that which experiment requires.

Conceding to M. Laplace the above law, which I think it is plain cannot be correct, he finds by some ingenious considerations, P being the pressure on any point, and $2\pi\,\mathrm{H\,K}$ an invariable factor, that

$$\mathrm{P} = 2\pi\,\mathrm{H\,K}\,\varrho^2 c^2; \quad (1)$$

a theorem which of itself expresses nothing that I know of in the laws of gases.

This

This theorem, combined with another which he immediately deduces, includes, he says, "les lois générales des fluides élastiques."

"Let us imagine," proceeds M. Laplace, "that the envelope and the contained gas have a common temperature t. It is manifest that any molecule whatever of this gas will every instant be struck by some of the calorific rays emitted by the surrounding bodies. A part of these rays it will stifle; but to maintain the temperature unchanged, it must radiate as many rays as it stifles. In any other space of the same temperature the molecule will be struck by the same quantity of calorific rays; the same part of which as before it will absorb and replace by its radiation. The quantity of calorific rays therefore which any given surface at every instant receives, is some function of the temperature alone, and independent of the surrounding bodies: I shall denote it by $\Pi(t)$. Hence the extinction will be $q\Pi(t)$, q being a constant factor depending on the nature of the molecule or of the gas. I will here observe that the quantity of rays emitted by the surrounding bodies, and which constitutes the free caloric of space, is, on account of the extreme velocity we must necessarily assign those rays, but a very insensible part of their whole caloric; which is otherwise manifest from the experiments made to condense it. Now in whatever manner the caloric of the *surrounding molecules acts by its repulsion* on the caloric of any particular molecule of gas, *to detach* a part of this caloric and make the molecule *radiate*, it is evident that this radiation will be in a ratio compounded of the density of the gas surrounding the molecule, or of ϱc and the caloric c contained in the molecule. It will therefore be proportional to ϱc^2: which is consequently proportional to the extincton $q\Pi(t)$; so that we may suppose,

$$\varrho c^2 = q'\Pi(t); \quad (2)$$

q' being a constant factor depending on the nature of the gas, and $\Pi(t)$ a function of the temperature independent of this nature."

These are the arguments by which M. Laplace attempts to establish his equation 2. If for the sake of brevity we pass over the first conclusion, namely, that the radiation to the molecule is independent of the surrounding bodies and some function of t, which if rigidly considered is probably not so evident as M. Laplace seems to think it; philosophers will, I presume, hardly then grant the latter conclusion, that the radiation of a molecule is proportional to $\varrho c \times c$. We might also easily show that surrounding molecules tend rather by their repulsion to compress the caloric of an inclosed molecule closer towards the centre, than to disperse it; but this too we

will

will pass over. Granting that the caloric of the surrounding molecules acts in some way by its repulsion to make the central molecule radiate, it is plain by the course M. Laplace himself takes, that he considers this action proportional to the quantity of caloric acted on, and to the intensity of repulsion of the surrounding caloric. The radiation must therefore be as $c \times \varrho c \varphi(r)$ nearly*; r being the distance of two molecules, and $\varphi(r)$ the intensity of repulsion of a particle of caloric at the distance r. But a being some constant $r = \sqrt[3]{\frac{a}{\varrho}}$; and therefore by M. Laplace's own principles his equation 2 should be,

$$\varrho c^2 \phi\left(\sqrt[3]{\frac{a}{\varrho}}\right) = q' \Pi(t) \quad \text{(A)}$$

instead of, $\varrho c^2 = q' \Pi(t)$.

In the third part of his paper, M. Laplace introduces the function $\phi(r)$; but drops it in the final equation without giving any reason whatever. From what I can perceive, he seems to involve it in the constant coefficient q. If so, it appears to me to be utterly repugnant to his own principles and definition of this supposed constant; for he distinctly tells us that q is "un facteur constant dépendant de la nature de la molécule ou du gaz;" and therefore it ought to be the same for every density of the same gas; since neither the nature of the molecule or gas is changed by a change of density.

It is the error of neglecting this function which has enabled the marquis to bring out his conclusions independently of the law of repulsion in his 2nd part, and of the laws of repulsion and attraction in the 3rd part—conclusions which at the first glance of his paper forcibly struck me as strongly indicative of errors somewhere. That we are not justified in neglecting that part of A which depends on φr, will appear from the consideration, that a molecule of gas is made, by M. Laplace's views, to radiate its caloric by the repulsion of the caloric of other molecules surrounding that molecule. And as he assumes that this sphere of repulsion is insensibly small, the entire action of the whole molecules within this sphere must be some function of r, the distance of two molecules; and therefore some function of the density ϱ.

Equation A combined with 1, would produce results at variance

* The correct value of the factor depending on $\varphi(r)$ is $2\pi \int r^2 \varphi r dr$ taken from $r = o$ to $r = \infty$.

I might here make a remark very useful in investigations of this kind, and which I do not remember to have seen elsewhere. If $\varphi(r)$ be such a function of r that it is sensible only with insensible values to r; and if $f(r)$ be any other function of r, finite always when r is finite, and such that the value of $f(r) \times \varphi(r)$ decreases as r increases, $\int f(r) . \varphi(r) . dr = o$ when $r = \infty$.

variance with phænomena almost whatever form we may give to the function φ. It is however not my intention to pursue them. My object has been merely to show, that M. Laplace's principal and fundamental equations are erroneously deduced from his principles; and consequently that his subsequent conclusions are not consequences of what he first assumes.

It appears to me to be evident that the equations he has produced are more the offspring of a previous knowledge of what they should be from the phænomena, than of that sound reason which his other works usually manifest. Had the principles he sets out with been given him, namely, that there is such a thing as caloric, which, while strongly repulsive of its own, attracts and is attracted by all other matter; which by some means radiates in extremely minute portions with a great velocity; which attaching itself in considerable quantities to particles of matter overcomes their mutual attraction, and occasions them to stand at the greatest distance the envelope admits from each other;—had, I say, these things only been given him withoutv any knowledge of what the phænomena require, I would enture to appeal to himself, whether, with his mind so unacquainted, unbiassed, and unprejudiced with the facts in question, his results would not have been very different to what they are. Now, so far from this having been the case with myself, I was not even acquainted with any other law of airs than that of Mariotte, when my theories of collision and of aëriform bodies were first laid down. It was not until some time afterwards that I knew any thing of MM. Gay Lussac and Dalton's law; which, from the awkward synthetical course I pursued, I had some difficulty in demonstrating. Nor was I acquainted with the law that the pressure of a mixture is equal to the sum of the pressures of the component airs, until after my theory had been published; when I accidentally met with it in Biot's *Traité de Physique* while looking over the theory of vapours. The theory of latent heat, and particularly that of evaporation, was investigated under circumstances incomparably more disadvantageous. Examples such as these of correct fertility are, I believe, never to be met with where nature and theory are at variance.

It is rather curious that M. Laplace has in effect brought out the same point of absolute cold that I had. He says that all the caloric in a mass of any air at the centigrade zero, is equal to $266\frac{2}{3}$ centigrade degrees, or to $266\frac{2}{3}\times\frac{9}{5}=480°$ Fahr.; the precise quantity that I had given.

With respect however to the Marquis de Laplace's theory and mine, there are cases by which the fate of both may be

decided by experiment. According to his conclusions, the march of an air thermometer is a correct indicator of the increase of caloric; and according to my theory the said march is proportional to the difference of the squares of the true temperatures. If therefore equal weights of any two bodies were mixed at the Fahr. temperatures 32° and 212°, by his theory the resulting temperature should be 122°, the arithmetical mean; and by mine $118\frac{1}{2}^\circ = \left(\frac{\sqrt{448+32} + \sqrt{448+212}}{2}\right)^2 - 448^\circ$. With a greater interval of temperature, a greater difference would exist. Now as far as experiments go on this subject, they are unequivocally in my favour. Every philosopher who has tried such an experiment has, I believe, found the resulting temperature beneath the arithmetical mean. Even Crawford, who made experiments in a manner the most unaccountable for any one who had hopes of success, found his results less than M. Laplace's theory would give; and by the only two of De Luc's experiments that I have yet seen, there are variations from M. Laplace's theory of 3° and $2\frac{1}{2}$° in defect, and from mine of only $\frac{3}{5}$° and $\frac{1}{10}$° in excess. I have also made some experiments on this subject myself, which accord with the conclusions I have drawn equally as well as De Luc's; but in consequence of being deprived of the means of deciding the value of some corrections through a material accident to my apparatus, I have not yet been able to put them in a condition for publication.

It were much to be wished that some decisive experiments of this kind were undertaken by those who have proper apparatus and opportunities. A determination of the true quantity of deflection from the arithmetical mean of equal weights of the same body mixed at unequal temperatures, would at once settle the grand point respecting the real indications of thermometers; and consequently establish laws of the highest importance in science.

Cranford, July 19th 1823. J. Herapath.

XIII. *Notices respecting New Books.*

Recently published.

The First Part of the Transactions of the Philosophical Society for 1823 has just appeared, and the following are its contents:

The Croonian Lecture. Microscopical Observations on the Suspension of the muscular Motions of the Vibrio Tritici. By Francis Bauer, Esq.— On Metallic Titanium. By W. H. Wollaston, M.D.—On the Difference of Structure between the

the human Membrana Tympani and that of the Elephant. By Sir Everard Home, Bart.—Corrections applied to the Great Meridional Arc, extending from Latitude 8° 9′ 38″,39, to Latitude 18° 3′ 23″,64, to reduce it to the Parliamentary Standard. By Lieutenant Colonel W. Lambton.— On the Changes which have taken place in the Declination of some of the principal Fixed Stars. By J. Pond, Esq.—Appendix to the preceding Paper on the Changes which appear to have taken place in the Declination of some of the Fixed Stars. By John Pond, Esq.—On the Parallax of α Lyræ. By John Pond, Esq.—Observations on the Heights of Places in the Trigonometrical Survey of Great Britain, and upon the Latitude of Arbury Hill. By B. Bevan, Esq.—On some Fossil Bones discovered in Caverns in the Limestone Quarries of Oreston. By Joseph Whidbey, Esq. To which is added, a Description of the Bones by Mr. Wm. Clift.—On the Chinese Year. By J. F. Davis, Esq.—Experiments for ascertaining the Velocity of Sound, at Madras in the East Indies. By John Goldingham, Esq.—On the double Organs of Generation of the Lamprey, the Conger Eel, the common Eel, the Barnacle, and Earth-Worm, which impregnate themselves; though the last from copulating, appear mutually to impregnate one another. By Sir Everard Home, Bart.

Description of a Railway on a new Principle, with a Table of the comparative Amount of Resistance on several now in use; also an Illustration of a newly observed Fact relating to the Friction of Axles; and a Description of an improved Dynamometer. By Henry R. Palmer, Civil Engineer, Member of the Institution of Civil Engineers. J. Taylor. 1823. pp. 60. 8vo.

It has often been remarked that those things which succeed in model often fail entirely when tried on a large scale; and it is equally true that many things succeed on the large scale which promise very little when tried in model; one of the most striking examples of the latter kind is a ship. What can afford less prospect of stability than experiments made on a small floating body?—what individual, not deeply versed in the theory of statics, would have ventured to construct a ship on such experiments? The machine described in the pamphlet before us is of a similar nature: in a drawing, or small model, it seems as if nothing less than the most nice equilibrium would be required in practice for its success; but its author has wisely anticipated such an objection by making a working model on a large scale, which renders the stability and advantage of the contrivance manifest.

 This

This new railway consists of a single rail supported by equidistant pillars; it is raised above the common surface of the ground to the height of about 3 feet; and the load is suspended from the axles of two wheels which run one before another on the upper edge of the rail. The construction of the frame is such, that the centres of suspension of the load are below the virtual centre of support; therefore the equilibrium is always *stable*, the virtual centre of support being equivalent to the metacentre of a ship*. But since the load on each side of the rail may happen to be unequal, the change of position, which would be a necessary consequence of this unequal distribution, is counteracted by the breadth of the surface of the rail; and the stability may be greatly increased, if necessary, by adding braces to the bars which suspend the load to the axles.

By adopting this railway, a perfectly even and adjustable plane with a very small degree of resistance may be obtained: elevated so as to be free from dust, or other extraneous matters, "no further preparation is requisite for the moving power than an ordinary towing-path. The horse draws by a towing-rope connected to the carriages, and proceeds on one side of the rail; and as his height will vary with the natural undulations of the surface of the ground, he will sometimes be below the surface of the rail, and is consequently provided with a length of rope which allows a considerable variation of height without much altering the angle of traction."

The various contrivances that may be employed to render loading and unloading, passing, crossing roads, brooks, rivers, ravines, &c. easy and simple, are fully described, and illustrated by two beautiful engravings. But the author has not confined his work to a description of his ingenious railway alone; he gives some interesting particulars respecting other kinds of railways, with a table of experiments showing the effect produced by a given power on different kinds of rails. Also some very curious remarks on the friction of axles, and a description of an improved dynamometer on a principle analogous to that which Coulomb employed to check the irregular oscillations of delicately suspended needles in his experiments on magnetism.

It appears, from the experiments described in the work, that, allowing the force of a horse to be 150lbs, one horse will be capable of drawing about 20 tons upon a level railway on Mr. Palmer's principle; on a good hard and level turnpike road a horse of the same power could not draw more than about

* Our readers will find this principle of stability illustrated in a recent work on the Elements of Natural Philosophy, by Professor Leslie, where it is applied to explain the curious phænomena of rocking or laggan stones.

about $1\frac{1}{2}$ ton; in both cases we suppose the weight of the carriages to form part of the load. A horse will therefore produce 13 times the effect on the new railway that he could upon a turnpike road. **.

A Second Edition of Dr. Ure's Dictionary of Chemistry, on the basis of Mr. Nicholson's, has just made its appearance. We spoke favourably of the First Edition in our 57th volume. The reception it has met with (a large impression having been sold off within two years) justifies the opinion we then expressed of the work. The new Edition, besides several minute corrections, contains numerous additions which, very judiciously, the author has rendered obvious by marking them with a double asterisk. The following are among the new articles: *Acids*—Butyric, Cevadic, Cholesteric, Delphinic, Ellagic, Formic (factitious), Hydroselenic, Hyponitrous, Igasuric, Iodo-sulphuric, Nitro-lucic, Nitro-saccharic, Phosphatic, Pyromucic, Pyrocitric; Acrospire; Aphanite; Carinthine; Chlorides of Carbon; Chinconina; Electro-magnetism; Geology; Gieseckite; Inulin; Iolite; Kollyrite; Lievrite; Chloride of Lime; Liquefaction of Gases; Moirée Metallique; Peliom; Piperine; Quinina; Soap; Tea; Trachyte.—Besides these additions, considerable insertions have been made in some of the articles of the former edition. The work as a whole reflects high credit on the author both in respect of his knowledge and his industry.

Preparing for Publication.

In the course of the ensuing month will appear the first two volumes of The English Flora; by Sir J. E. Smith, President of the Linnæan Society, &c. &c. In 8vo.—So much has been done in Botany since the publication of the learned President's *Flora Britannica* and *English Botany*, especially with regard to natural affinities; and he has for 30 years past found so much to correct in the characters and synonyms of British plants, that this is announced as an entirely original work. The language also will be reduced to a correct standard; the *genera* reformed, and the *species* defined, from practical observation. We are well persuaded that the expectations of British botanists will not be disappointed.

Also, in one vol. 8vo, An easy Introduction to Lamarck's Arrangement of the Genera of Shells; with illustrative Remarks, additional Observations, and a synoptic Table. By Charles Dubois, F.L.S.

Berthollet on Dyeing; translated from the last Parisian edition: with Notes, by Andrew Ure, M.D. F.R.S. &c. It is expected that this work will appear at the end of autumn.

XIV. *Pro-*

XIV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

June 6.—A PAPER was read containing Remarks on Sections presented by the Rivers Isla, Melgum, Proson, and S. Esk in the county of Forfar, with some general Observations on the Geology of that County: accompanied with Specimens; by Charles Lyell, Esq. Sec. G.S.

The county which formed the principal subject of this communication is situated on the southern flank of the Grampians; it is occupied by old red sandstone, grauwacke and argillaceous schist, with their associated porphyries. The strata are clearly exposed by the rivers that cut through them. They are very highly inclined, and dip for the most part towards the south. The old red sandstone may be described as consisting of two formations of sandstone, with a formation of conglomerate of great thickness interposed between them. An extensive formation of felspar porphyry occurs in the lower part of the conglomerate; and it is from the broken and rolled fragments of this porphyry that the conglomerate is for the most part composed. Between the porphyry and the conglomerate a rock prevails of a mixed character, which seems intermediate between the two, and which it is difficult to describe or account for. The lower red sandstone which is beneath the conglomerate is in many parts seen to be traversed by a mass or dyke of greenstone, which passes into serpentine, in which form it continues through a great part of its course; it lies parallel with the strata. The lower red sandstone, which is for the most part schistose and not of great thickness, alternates with grauwacke at its juncture, and the grauwacke with argillaceous schist. A large mass of porphyry, resembling that of the Elvans of Cornwall, intersects in one part of the district the superior beds of the grauwacke formation. The paper concludes with some observations on the primary rocks of the Grampians in the county of Forfar.

June 20.—A Notice was read On some fossil Bones of an Ichthyosaurus, from the Lias near Bristol; also On two new Species of Fossil Teeth: by George Cumberland, Esq. Hon. Mem. G.S.

A Letter was read, accompanying some Specimens from Stonehenge, by Godfrey Higgins, Esq.

An Extract of a Letter was read from Lieut. J. Short, R.E., addressed to and communicated by Dr. Babington, Pres. G.S., containing some Remarks on the Isle of Bourbon.—The Isle of Bourbon, which is situated about 120 miles from the Mauritius,

Mauritius, and is 150 miles in circumference, appears to be chiefly of volcanic composition. An active volcano still exists. Although beneath the tropics, perpetual snow and ice cover the summits of some of the mountains, which rise to an elevation of 10,000 feet. Lieut. Short observed basaltic columns of great height exposed in some parts of the island, and found olivine, lava, zeolite and puzzolana abounding throughout the rocks.

A Notice was read respecting the Pebbles in the Bed of Clay which covers the new red Sandstone in the South-west of Lancashire, by John Bostock, M.D. V.P.G.S.

A Paper was read containing a Description of a Section of the Crag Strata at Bramerton, near Norwich, by Richard Taylor, Esq. Communicated by John Taylor, Esq., Treas. G.S. —This Paper was accompanied by a Sketch of the Crag Beds at Bramerton, resting upon the upper Chalk, and a Table was subjoined containing the respective thicknesses of the series of Beds, with a List of such organic Substances as belong to each.

A Paper was read On the Geology of Rio de Janeiro, by Alexander Caldcleugh, Esq. M.G.S.—The mountains in the neighbourhood of Rio de Janeiro are for the most part composed of gneiss, intersected by granite veins. A siliceous stalactite was observed by the author to form in this district, from the overhanging masses of gneiss; specimens of which were presented to the Society. As the absence of hot springs makes the occurrence of these stalactites of very considerable interest, Mr. Caldcleugh offers the following hypothesis to explain their formation: The water, which in Brazil constantly trickles down the bare sides of the hills, often reaches a temperature as high as 140° or 150° of Fahr.: this warm water descending on decomposing strata of gneiss, such as is the case with that from which these specimens are taken, seizes the potash of the felspar, and then acts upon the quartz, and forms a siliceous stalactite. Some of the hot springs, or geysers, of Iceland do not reach the boiling point, and perhaps the quantity of silex dissolved, the inverse of what is shown to be the case with carbonate of lime, may in a great measure depend on the temperature of the alkaline solvent.

June 27.—A Paper was read, entitled "Observations on the Quartz Rock Mountains of the West of Scotland and North of Ireland, more particularly those of Jura, with an Account of the ancient Beaches and Trap Dikes of that Island, accompanied by a Plan and Sections."—The quartz rock is traced in a succession of districts, from Lerwick in Shetland to the county of Donegal in Ireland, and in Jura the thickness of the mass is estimated at 10,260 feet. The similarity and

singularity

singularity of form assumed by quartz rock mountains, in districts remote from each other, is deduced from the peculiar construction and material of the mountain mass, acted upon by powerful aqueous currents. Quartz rock is of great extent in the county of Donegal, where in one instance it rests immediately on granite; and at the murkish mountain contains a bed of pure siliceous sand of considerable thickness.—The author proceeds to notice the ancient beaches of Jura, which appear hitherto to have escaped observation: these occur on both shores of Loch Tarbert, and are disposed in six or seven terraces, rising regularly from the present shore, above which the highest is elevated about forty feet: the breadth occupied by these beaches in some instances amounts to three-fourths of a mile, and their line or extent has been traced eight or ten miles.—The author concludes with a description and remarks on the trap dikes of Jura: these are extremely numerous, and remarkable for preserving courses nearly parallel to each other, and nearly in the line of dip of the quartz rock which they traverse; which gives occasion for offering some reasons to account for that particular disposition.

ROYAL ACADEMY OF SCIENCES OF PARIS.

March 31.—M. de la Borne exhibited some apparatus for augmenting the Voltaic effects produced in the experiment of M. Seebeck. They consist of a series of bars, alternately of brass and of iron; of conductors of different sizes, one of which is reduced in the middle to a very small thickness, and which are to form a communication between the two ends of the preceding apparatus; and of one piece to form a communication by very fine wires of different lengths.

April 7.—The astronomical observations made at the Observatory at Paramatta by Major-General Sir Thos. Brisbane, Governor of New Holland, and by M. Rumker, were received. —Sir T. Brisbane, in his last letter, speaks in high terms of the climate of the colony, and expresses his wish that some members of the Institute would visit a country which abounds in objects of scientific research. He also states that he is forming a collection of rarities which he intends to present to the Jardin du Roi, and that he is making preparations for the measurement of an arc of the meridian.

M. Arago communicated a letter, in which M. Duperrey, now on a voyage in the corvette *la Coquille*, gives an account of some magnetic observations made by him at sea, and at the isle of St. Catherine.

M. de le Borne presented a memoir, entitled A Thermal Electrometor, and formulæ representing its effect.

XV. *In-*

XV. *Intelligence and Miscellaneous Articles.*

MR. MURRAY ON THE COMBINATION OF PHOSPHORUS WITH SULPHURET OF CARBON AS CONNECTED WITH AN INSTANTANEOUS LIGHT, &c. &c.

THE compound resulting from the solution of phosphorus in sulphuret of carbon is one, I should think, capable of being advantageously employed to ascertain slight increments of temperature, in cases where it might be difficult to employ other means; and also as an instantaneous light.

It is on these accounts I would now advert to this interesting compound, and to illustrations corroborating the relations referred to.

The sulphuret of carbon dissolves, it is known, a very considerable quantity of solid phosphorus and still remains liquid. At a temperature of even *minus* 80° F. it inflames—a few crystals of chlorate potassa triturated in contact with a very small portion of the triple compound is accompanied by a violent explosion and inflammation.

Posited on the end of the condensor in contact with *amadon*, &c. the simplest pressure, even that of a *finger*, will be sufficient to *inflame* it. It kindles on the gentlest friction.

The employment of phosphorus *per se* in experiments of the preceding description is to be deprecated as dangerous, because ignited portions are dispersed, burning with great violence, and often inflicting serious injury on the operator; whereas in the triple compound referred to no such dispersion ensues. In the small condensing machine for instantaneous light, a considerable force is necessary as well as a peculiar management: tipt with this inflammable material, the experiment may be made in a *glass tube*.

Dropt into chlorine, it exhibits *immediate* flame.

A slip of paper partially moistened with it, on its transit from a medium of *nitrous oxide* into the *free atmosphere*, inflames.

A bit of paper dipped into the inflammable liquid, and brought in contact with the *iodide* or *chloride of azote*, is instantly set on fire, and these violent compounds explode with great force.

When a similarly supplied slip is brought *very near* a portion of *fulminating silver*, and this last is touched with sulphuric acid, explosion ensues, and the inflammable liquid is instantly kindled.

As far as I have been able to ascertain the fact experimentally, it should seem that the *light* which accompanies the separation and expansion of elemental forms, as in the *chloride of azote*, possesses an increment of temperature; and it ap-

 pears

pears to me not improbable that all *light*, however *attenuated*, and by what means soever elicited, is thus attended.

Buxton, 21st May, 1823. J. MURRAY.

MR. MURRAY'S NOTE ON THE INFLUENCE OF HEAT ON MAGNETISM, &c.

Some interesting experiments made before the Royal Society of Edinburgh about the middle of last month, amply confirm the phænomena which I have already described touching the influence of heat in the deflection of the needle from the magnetic plane. No question can now arise, and I feel gratified in having been the first to elucidate and to excite attention to the connexion obtaining between caloric and magnetism.

In reference to the same subject, I quote with pleasure the following from some judicious remarks on the magnetic needle published at Genoa by the Baron de Zach.

"Si mette una bussola fra due calamite, si lasciano cadere i raggi del sole sulla calamita collocata all' est, l'ago se ne allontanerà all' ouest quando sarà scaldata la calamita. Il contrario avrà luogo, scaldando la calamita collocata all' ouest."

The steel bar presented to me by Professor Morrichini when at Rome, and which had been magnetized by the violet ray of the spectrum, has since 1818 preserved its polarity, and while I now write vibrates in the magnetic plane: it is however not undeserving of remark here, that while its north pole is attracted by the south pole of another needle, and repelled by a north one, its *south* pole is indiscriminately *attracted* by *either pole* of a needle, and would thus seem *null*. May we anticipate the *separation* of the two poles, and their insulated and independent exhibitions? That time *may* come.

It would appear from the excellent remarks of the Baron de Zach already quoted from, that Governor Ellis has not been the *exclusive observant* of the influence of *cold* on the compass. Perhaps the fact is to be found in the Transactions of the Royal Society of London: but not having the records of that learned body at this moment at hand, I quote again: "Il capitano inglese *Middleton* traversando nel 1737 la bajo di *Hudson*, in mezzo ad immensi ghiacci galeggianti, trovò che tutti gli aghi delle sue bussole aveano perduto il loro movimento, e che si fermavano indifferentamente in tutte le direzioni qualunque, nelle quali si collocavano col dito. Egli portò una di queste bussole agghiacciate nel suo camerino, ma essa non riprese il suo moto se non dopo averla messa vicino al fuoco, e dopo averla ben riscaldata per un quarto d'ora: allora soltanto si girò nella direzione magnetica. Il capitano fu cosi obligato di scaldare ogni mezz' ora tutte le sue bussole," &c. I am yours, &c.

Stranraer, N.B., 10 July 1823. J. MURRAY.

ACID EARTH OF PERSIA.

Lieut. Col. Wright of the royal engineers, who lately came over land from India, brought a small quantity of this natural production from Persia. The natives apply it to the same uses that lemons and limes are used for elsewhere, namely, to make their sherbets, of which considerable quantities are used, they being prohibited the use of wine. The acid earth is found in great quantities at a village called Daulakie, in the south of Persia, between three and four days' journey from Bushire on the Persian Gulf.

Some analytical experiments made on a few grains of it by W. H. Pepys, Esq. gave the following results:—About one fifth is soluble, by trituration, in boiling distilled water. The solution changes litmus paper and solution of cabbage red. It yields copious precipitates with nitrate and with muriate of barytes—indicating the presence of sulphuric acid. The triple prussiate gave a strong blue precipitate; and the sulphuret of ammonia a copious blackish brown precipitate—proofs of the presence of iron. The solution, when evaporated nearly to dryness, yielded crystals, which by their figure and taste seemed to be acidulous sulphate of iron. The earthy matter was not examined,—the principal aim of the experiments being only to ascertain the nature of the free acid in a product so abundant, where it is found, that it might be taken up in cart loads.

VARIETIES OF THE LYNX IN THE NORTH OF EUROPE.

The subjoined observations on some varieties of the Lynx occurring in the north of Europe are extracted from Mr. De Capell Brooke's Travels through Sweden, Norway, and Finmark, to the North Cape; a work which contains many interesting notices respecting the zoology of those countries. They may assist, perhaps, in removing from the history of the smaller feline animals principally characterized by having tufted ears, a portion of that obscurity which seems at present to pervade it.

"The lynx of the north, the tiger of the polar countries, is not rare in this part of Norway (the province of Drontheim). In the Norwegian language it is called *goupe*, and in the north of Sweden it is generally known by the name of *warjelue*. From the skins of this animal, that were shown to me in different parts of Norway and Lapland, three of which differed very materially in their colour, it seems that there are at least as many species or varieties of the lynx. Of one of these Mr. Knudtzon had several. The largest measured five feet in length, not including the tail, which did not exceed an inch and a half. The colour of them all was gray, with a yellowish tinge, beautifully marked with dark spots, and the ears were tufted.

tufted. The general price they brought at Drontheim was about five specie dollars, or a pound sterling. This seems to be more peculiar to Norway, as I never observed it during my subsequent travels. Of the two others, which I met with in Lapland and Sweden, one that I saw at Umea measured from the muzzle to the beginning of the tail five feet eleven inches, and the tail was hardly two inches. The appearance of the skin in every respect so much resembled that of the leopard, that I should have suspected it to have belonged to this animal, had it not been for its tufted ears, and the length and superior thickness of the fur. The third species which I met with in Swedish Lapland differed also materially from the other two, being of a uniform reddish-brown colour. In length it exceeded five feet. This, which I imagine to be the same as the North American lynx, and the animal most commonly known by the name of the lynx, I have seen alive in collections in this country, though of a much smaller size, being in appearance not unlike a large cat, but much more robust, and of a thicker make. The variety of names given to the lynx has tended greatly to perplex naturalists, and been the occasion of much confusion respecting this animal; and it seems singular, that although it has been represented as comparatively of small size, the dimensions of those above described would place it on an equality with the panther; and in length it would greatly exceed both the leopard and the ounce, though its height, which hardly equals that of the wolf, may cause it to appear more diminutive. Its claws, which are not much inferior to those of the tiger, must render it a very formidable antagonist. In the northern forests it preys chiefly upon game, not only winged, but four-footed; and should it chance to come near the abode of man, it will make great ravages in the sheepfold of the farmer."

MR. BELZONI'S PROGRESS IN AFRICA.

It must be known to many of our readers that this enterprising traveller, who made so many valuable discoveries in Egypt and Nubia, is now on another journey in Africa. The following letter was lately received from him by a gentleman belonging to the University of Cambridge:

"Fez (Capital of Morocco), 5th May.

"In the short letter I wrote to you from Tangier, dated the 10th of April, I informed you that I had gained permission from His Majesty the Emperor of Morocco to enter his country as far as Fez, and that I had great hopes of obtaining his permission to penetrate further south. I stated also, notwithstanding the great charges upon my purse, unsupported as I am,

am, and relying entirely on my own resources, that nothing should be left undone before I quitted my attempt. I have now great pleasure in acquainting you, my dear friend, of my safe arrival at Fez, after having been detained at Tangier till a letter had been forwarded from Mr. Douglas, His Britannic Majesty's Consul at Tangier, to the Minister at Fez, to obtain permission from the Emperor for me to approach his capital. As soon as a favourable answer was received, we started for this place, and in ten days arrived here in safety with my *better half*, who, having succeeded in persuading me to take her as far as Tangier, has also enforced her influence to proceed to Fez; but this, though much against her will, must be her *ne plus ültra*. Yesterday, I had the honour to be presented to His Majesty the Emperor, and was highly gratified with his reception of me.—He was acquainted that I had letters of introduction from Mr. Wilmot Horton to the Consul in Tangier, from whom I received the greatest hospitality, and who did all in his power to promote my wishes. The fortunate circumstance of my having known the Prime Minister of His Majesty whilst at Cairo, on his return from Mecca to this country, is also much in my favour; and though a great deal has been said against my project by the commercial party, particularly by the Jews of this country, who monopolize all the traffic of the interior, I obtained His Majesty's permission to join the caravan, which will set out for Timbuctoo within one month. If nothing should happen, and if promises are kept, I shall from this place cross the mountains of Atlas to Taflet, where we shall join other parties from various quarters, and from thence, with the help of God, we shall enter the great Sahara to Timbuctoo. Should I succeed in my attempt, I shall add another '*votive tablet*' to the Temple of Fortune; and if, on the contrary, my project should fail, one more name will be added to the many others which have fallen into the river of Oblivion. Mrs. Belzoni will remain at Fez till she hears of my departure from Taflet, which place is 18 or 20 days' journey from hence*; and as soon as that fact is ascertained, she will return to England."

OBITUARY.—Lieut.-Col. William Lambton.

We have again to perform a painful duty, in recording the loss of an officer of distinguished worth and high talent. Lieut. Colonel William Lambton, superintendent of the Grand Trigonometrical Survey in India, died on the 20th ult. while proceeding in the execution of his duty from Hydrabad towards Nagpoor, at Hingin Ghaut, 50 miles south of the latter place.

The Annals of the Royal and Asiatic Society bear ample testimony

* Taflet is 340 miles south of Fez.

testimony to the extent and importance of the labours of Colonel Lambton, in his measurement of an arc of the meridian in India, extending from Cape Comorin, in lat. 8. 23. 10. to a new base line, measured in lat. 21. 6., near the village of Takoorkera, 15 miles S.E. from the city of Ellichpore, a distance exceeding that measure by the English and French Geometers, between the parallels of Greenwich and Tormentara in the Island of Minorca.

It was the intention of Colonel Lambton to have extended the arc to Agra, in which case the meridian line would have passed at short distances from Bhopaul, Serange, Nurwur, Gualiar, and Dholpore. At his advanced age, he despaired of health and strength remaining for further exertion; otherwise it cannot be doubted that it would have been a grand object of his ambition to have prolonged it through the Dooab, and across the Himalays, to the 32d degree of north latitude. If this vast undertaking had been achieved, and that it may yet be completed is not improbable, British India will have to boast of a much larger unbroken meridian line than has been before measured on the surface of the globe.

Though the measurement of the Arc of the Meridian was the principal object of the labours of Colonel Lambton, he extended his operations to the East and West, and the set of triangles covers great part of the Peninsula of India, defining with the utmost precision the situation of a very great number of principal places in latitude, longitude, and elevation; and affording a sure basis for an amended Geographical Map, which is now under preparation. The triangulation also connects the Coromandel and Malabar coasts in numerous important points, thus supplying the best means of truly laying down the shape of those coasts, and rendering an essential service to navigation.

It was the Colonel's intention to have himself carried the meridian line as far north as Agra, and he detached his first assistant, Captain Everest, of the Bengal Artillery, to extend a series of triangles westward to Bombay, and when that service should be completed eastward to Point Palmyras, and probably Fort William, by which extensive and arduous operation, the three Presidencies of India would be connected, and several obvious advantages gained to geography and navigation. But it is in the volumes of the proceedings of various learned Societies, that the accounts of the labours of this veteran philosopher, whose loss we lament, must be looked for, and who for 22 years carried on his operations in the ungenial climate with unabated zeal and perseverance, and died full of years and conscious of a well deserved reputation.—*Madras Gazette, Feb.* 25, 1823.

LIST

LIST OF NEW PATENTS.

To John Moncrieffe Willoughby, of Fair-street, Horsleydown, Surrey, gentleman, for certain improvements in the construction of vessels, so as to enable them to sail with greater velocity.—Dated 26th June, 1823.—6 months allowed to enrol specification.

To John Green, of Mansfield, Nottinghamshire, whitesmith, for certain machines used for roving, spinning, and twisting cotton, flax, silk, wool, or other fibrous substances.—26th June.—6 months.

To William Vere, of Crown-row, Mile-end Old-town, in the parish of Stepney, Middlesex, engineer; and Henry Samuel Crane, of Stratford, in the parish of Westham, Essex, manufacturing chemist; for their improvements in the manufacture of inflammable gas.—30th June.—6 months.

To Thomas Wolrich Stansfeld, of Leeds, Yorkshire, worsted manufacturer; Henry Briggs, of Luddendenfoot, in the parish of Halifax, in the said county, worsted manufacturer; William Richard, of Leeds aforesaid, engineer; and William Barraclough, of Burley, in the parish of Leeds aforesaid, worsted manufacturer; for their improvements in the construction of looms for weaving fabrics composed wholly, or in part, of woollen, worsted, cotton, linen, silk, or other materials, and in the machinery and implements for, and methods of working the same.—5th July.—6 months.

To George Clymer, of Finsbury-street, Finsbury-square, Middlesex, mechanic, for certain improvements in agricultural ploughs.—5th July.—6 months.

To John Fisher, of Great Bridge, in the parish of Westbromwich, Staffordshire, iron-founder, and John Horton the younger, of the same place, manufacturers of steam boilers, for improvements in the construction of boilers for steam engines, and other purposes where steam is required.—8th July.—2 months.

To Stephen Fairbanks, of the United States of America, but now residing in Norfolk-street, Strand, Middlesex, merchant, who, in consequence of a communication made to him by a certain foreigner residing abroad, is in possession of an invention of certain improvements in the construction of locks and other fastenings.—10th July.—6 months.

To John Leigh Bradbury, of Manchester, Lancashire, calico-printer, for improvements in the art of printing, painting, or staining silk, cottons, woollen, and other cloths, and paper, parchment, vellum, leather, and other substances, by means of blocks or surface printing.—15th July.—6 months.

To Bennington Gill, of Birmingham, Warwickshire, merchant, who, in consequence of a communication made to him by a certain foreigner residing abroad, is in possession of certain improvements in the construction of saws, cleavers, straw-knives, and all kinds of implements that require or admit of metallic backs.—15th July.—6 months.

To Sir Isaac Coffin, of Pall-mall, Middlesex, Baronet, admiral of the white squadron, who, in consequence of a communication made to him by a certain foreigner residing abroad, is in possession of a certain method or methods of catching or taking mackarel and other fish.—15th July.—6 months.

To William Palmer, of Lothbury, London, paper-hanger, for his improvements in machinery applicable to printing on calico, or other woven fabrics composed wholly or in part of cotton, linen, wool, or silk.—5th July.

A METEOROLOGICAL TABLE: *comprising the Observations of Dr.* BURNEY *at Gosport; Mr.* CARY *in London, and Mr.* VEALL *at Boston.*

GOSPORT, at half-past Eight o'Clock, A.M.								CLOUDS.							Height of Barometer, in Inches, &c.		Thermometer.				WEATHER.	
																	LONDON.					
Days of Month, 1823.	Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Cirrus.	Cirrocumulus.	Cirrostr.	Stratus.	Cumulus.	Cumulostratus.	Nimbus.	Lond. 1 P.M.	Bost. $8\frac{1}{2}$ A.M.	8 A.M.	Noon.	11 P.M.	BOSTON. $8\frac{1}{2}$ A.M.	London.	Boston.
June 26	29·71	62	$50\frac{1}{2}$	46	SW.	...	0·750	1	1	1	...	1	1	1	29·69	29·35	57	66	57	54	Fair	Rain
27	29·33	59	...	72	E.	0·15	·150	1	1	1	...	1	1	1	·34	29·15	57	66	57	56	Showery	Rain
28	29·36	60	$50\frac{3}{4}$	63	SW.	...	·025	1	1	1	...	1	1	1	·40	29·	60	66	58	57	Showery	Rain
29	29·70	62	...	57	SW.	...	·020	1	1	1	...	1	1	1	·76	29·35	60	66	57	55·5	Showery	Cloudy
30	29·98	61	51	49	NW.	·35	·015	1	1	1	...	1	1	1	30·00	29·65	56	68	60	53·5	Fair	Cloudy
☾ July 1	29·94	65	...	47	W.	...	......	1	1	1	...	1	1	...	·04	29·60	58	65	60	57	Showery	Cloudy
2	30·05	62	...	55	SE.	...	·015	1	...	1	...	1	1	1	·04	29·70	59	69	59	60	Fair	Cloudy, rain *p.m.*
3	30·10	62	...	49	N.	·45	·050	1	1	1	...	1	1	1	·12	29·75	55	66	57	56	Fair	Cloudy [morn.
4	30·05	59	...	54	SW.	...	·080	1	1	1	...	...	1	1	·03	29·75	56	64	58	53	Rain	do. rain early in the
5	30·01	63	...	57	W.	...	·035	1	1	1	...	1	...	1	·01	29·60	58	73	63	66	Fair	do. 2 *p.m.* 73·5
6	29·81	62	...	75	SW.	·25	·020	1	1	1	...	1	...	1	29·75	29·30	63	70	57	64	Fair	Fine brisk wind
7	29·79	61	51	55	W.	...	·060	1	1	1	...	1	1	1	·76	29·30	60	67	55	57·5	Showery	Cloudy
● 8	29·75	59	...	58	W.	...	·185	1	1	1	...	1	1	1	·71	29·30	54	60	52	54	Showery	Fine, heavy r. thun.
9	29·96	61	...	56	NW.	·40	......	1	...	1	...	1	1	...	30·02	29·55	55	66	60	56·5	Fair	do. [& light. *p.m.*
10	30·09	62	...	60	SE.	...	......	1	1	1	...	...	1	...	·10	29·65	60	70	62	60	Fair	Fine [rainbow
11	29·78	69	...	54	SW.	...	·020	1	1	1	...	1	...	1	29·75	29·30	66	71	58	66	Cloudy	do. rain *p.m.* with
12	29·79	60	...	59	SW.	·40	·010	1	1	1	...	...	...	1	·80	29·30	61	70	66	63	Fair	Cloudy brisk wind
13	29·80	58	51	50	SW.	...	·520	...	...	1	...	...	1	1	·82	29·30	66	67	62	64	Rain	Cloudy do.
14	29·76	63	...	59	SW.	...	......	1	1	1	...	...	1	1	·79	29·25	62	67	57	60·5	Stormy	Cloudy rain *p.m.*
☽ 15	29·84	64	...	54	SW.	·25	·355	...	1	1	...	1	1	1	·77	29·37	57	65	53	63	Showery	Fine heavy r. *p.m.*
16	29·70	60	...	53	W.	...	·345	1	1	1	...	1	1	1	·68	29·20	55	64	52	69	Showery	do. rain *a.m.* & *p.m.*
17	29·90	58	...	49	NW.	...	·090	...	1	1	...	1	1	1	·95	29·50	55	64	57	55	Fair	Fine
18	29·80	58	...	76	S.	·30	·115	...	1	...	...	...	1	1	·84	29·50	53	65	60	56	Rain	Cloudy
19	30·00	64	$51\frac{1}{2}$	60	W.	...	·095	...	1	1	...	...	...	1	30·00	29·56	60	60	62	61·5	Rain	Cloudy rain *p.m.*
20	30·03	67	...	64	SW.	...	·030	1	...	...	...	1	1	1	·06	29·50	63	73	64	69	Fair	Fine
21	29·70	65	...	77	SW.	·20	·040	1	1	1	...	1	1	1	29·72	29·25	66	67	59	65	Rain	Cloudy rain *p.m.*
22	29·88	61	...	58	W.	...	·135	...	...	1	...	1	1	1	·92	29·40	60	67	55	57·5	Fair	Fine do.
○ 23	29·66	62	...	72	SW.	...	·025	1	1	1	...	...	1	1	·56	29·26	55	65	55	56·5	Rain	Cloudy, showery all
24	29·74	62	...	55	NW.	...	......	1	...	1	...	1	1	...	·78	29·25	56	66	57	57·5	Fair	do. [day, th. & l. *p.m.*
25	29·86	61	$51\frac{1}{2}$	54	W.	·50	·150	1	1	1	...	...	1	1	·81	29·47	56	59	52	60·5	Showery	Cloudy

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XVI. *Reflections on Volcanos.* *By M.* Gay-Lussac. *Read before the Royal Academy of Sciences at Paris, May* 19, 1823 *.

BEFORE I offer to the public the following observations on volcanos, a subject which has so long presented a wide field for hypothesis and conjecture, I ought to premise that I am not in possession of all the knowledge necessary for its full discussion, and that I shall only take a brief and partial view of it, confining myself to certain questions upon which chemistry may throw some light, and which do not absolutely demand an acquaintance with geology. The subject is however one of considerable difficulty, and one which gives me a claim on the indulgence of my readers.

Two hypotheses may be formed as to the cause which produces volcanic phænomena. According to one of these, the earth remains in a state of incandescence at a certain depth below the surface (a supposition strongly favoured by the observations which have been recently made on the progressive increase of temperature in mines); and this heat is the chief agent in volcanic phænomena. According to the second hypothesis, the principal cause of these phænomena is a very strong and as yet unneutralized affinity existing between certain substances, and capable of being called into action by fortuitous contact, producing a degree of heat sufficient to fuse the lavas and to raise them to the surface of the earth by means of the pressure of elastic fluids.

According to either of these hypotheses, it is absolutely necessary that the volcanic furnaces should be fed by substances originally foreign to them, and which have been some how or other introduced into them.

In fact, at those remote epochs which witnessed the great catastrophes of our globe,—epochs at which the temperature of the earth must have been higher than it now is, the melted substances which it contained consequently more liquid, the resistance of its surface less, and the pressure exercised by elastic fluids greater,—all that could be produced was produced;

* *Ann. de Chimie et de Phys.* tom. xxii. p. 415.

duced; an equilibrium must have established itself, the agitated mass must have subsided into a state of repose which could no longer be troubled by intestine causes, and which can only now be disturbed by fresh contact between bodies accidentally brought together, and which were, perhaps, only added to the mass of the globe subsequently to the solidification of its surface.

Now the possibility of contact between bodies in the interior of the earth, the ascent of lava to a considerable height above its surface, ejections by explosion, and earthquakes, necessarily imply that those extraneous substances which penetrate into volcanic furnaces must be elastic fluids, or rather liquids capable of producing elastic fluids, either by means of heat which converts them into vapour, or by affinity which sets at liberty some gaseous elements. According to analogy, the only two substances capable of penetrating into the volcanic furnaces in volumes sufficiently large to feed them, are air, and water, or the two together. Many geologists have assigned to the air an important office in volcanos; its oxygen, according to them, sustains their combustion: but a very simple observation will suffice to overthrow this opinion entirely.

How, indeed, is it possible for the air to penetrate into the volcanic furnaces when there exists a pressure acting from within towards the exterior, capable of raising liquid lava, a body three times as heavy as water, to the height of more than 1000 *mètres*, as at Vesuvius, or even of more than 3000, as is the case in a great number of volcanos? A pressure of 1000 *mètres* of lava, equivalent to a pressure of 3000 *mètres* of water, or to that of about three hundred atmospheres, necessarily excludes the introduction of any air whatever into volcanos; and as this pressure subsists for a long series of years, during which the volcanic phænomena continue in the utmost activity, it follows that the air can have no share whatever in their production.

It is moreover evident, that if the air had a free communication with the volcanic furnaces, the ascent of lava, and earthquakes, would be impossible.

If the air cannot be the cause of volcanic phænomena, it is probable, on the contrary, that water is a very important agent in them.

It can hardly be doubted that water does penetrate into volcanic furnaces. A great eruption is invariably followed by the escape of an enormous quantity of aqueous vapour, which, being condensed by the cold which prevails above the summits of volcanos, falls again in abundant rains accompanied by terrific thunder, as was the case at the famous eruption of Vesuvius in 1794, which destroyed Torre del Greco. Aqueous vapours and hydrochloric gas have also frequently been observed

served in the daily ejections of volcanos. It is scarcely possible to conceive the formation of these in the interior of volcanos without the agency of water.

If we admit that water is one of the principal agents in volcanos, we must proceed to examine the real means by which it acts, upon either of the hypotheses we have just laid down concerning the heat of volcanic furnaces. If we suppose, according to the first hypothesis, that the earth continues in a state of incandescence, at a certain depth below its surface, it is impossible to conceive the existence of water at that depth; for the temperature of the earth having formerly been of necessity higher, its fluidity greater, and the thickness of its solid crust less than at the present time, the water must necessarily have disengaged itself from its interior and have risen to the surface.

If we wish therefore to give any air of probability to this hypothesis, and to maintain the importance of water as a principal agent in volcanos, we must assume that it penetrated from the surface downwards to the incandescent strata of the earth; but in order to come to this conclusion, we must suppose that it had a free communication with those strata, that it gradually acquired heat before it reached them, and that the vapour it produced compressed by the weight of its whole liquid column, obtained a sufficient elastic force to elevate the lavas, to produce earthquakes, and to cause all the other terrible phænomena of volcanos.

The difficulties obviously involved in these suppositions, and to which many others might be added, render the hypothesis that the heat of volcanos is to be attributed to the state of incandescence of the earth at a certain depth below the surface perfectly inadmissible. I must further remark that this incandescence is itself quite hypothetical; and that, notwithstanding the observations on the increase of temperature in mines, I regard it as extremely doubtful.

Upon the second hypothesis which we laid down, that the principal cause of volcanic phænomena is a very strong and as yet unneutralized affinity existing between certain substances, and capable of being called into action by fortuitous contact, it is necessary to suppose that the water meets, in the interior of the earth, substances with which it has an affinity so strong as to effect its decomposition and to disengage a considerable quantity of heat.

Now the lavas ejected by volcanos are essentially composed of silica, alumina, lime, soda, and oxide of iron;—bodies which, being all oxides and incapable of acting upon water, cannot be supposed to have originally existed in their present state in

 volcanos.

volcanos; and from the knowledge which has been obtained of the true nature of these substances, by the admirable discoveries of Sir Humphry Davy, it is probable that the greater part, if not all of them may exist in a metallic state. There is no difficulty in conceiving that by their contact with water they might decompose it, become changed into lava, and produce sufficient heat to account for the greater part of the volcanic phænomena. But as my object is not to construct a system, but, on the contrary, to examine the probability of the two hypotheses under consideration, and to direct the attention of future observers towards those facts which are most likely to throw light upon the causes of volcanos, I shall proceed to point out the consequences which must result from the adoption of the latter hypothesis. If water be really the agent which sustains the volcanic fires by means of its oxygen, we must admit, as a necessary and very important consequence, that an enormous quantity of hydrogen, either free or combined with some other principle, would be disengaged through the craters of volcanos. Nevertheless it does not appear that the disengagement of hydrogen is very frequent in volcanos. Although, during my residence at Naples in 1805, with my friends M. Alexander de Humboldt and M. Leopold de Buch, I witnessed frequent explosions of Vesuvius, which threw up melted lava to the height of more than 200 *mètres*, I never perceived any inflammation of hydrogen. Every explosion was followed by columns (*tourbillons*) of a thick and black smoke, which must have been ignited if they had been composed of hydrogen, being traversed by bodies heated to a temperature higher than was necessary to cause their inflammation.

This smoke, the evident cause of the explosions, contained therefore other fluids than hydrogen. But what was its true nature? If we admit that it is water which furnishes oxygen to volcanos, it will follow that, as its hydrogen does not disengage itself in a free state, it must enter into some combination. It cannot enter into any compound inflammable by means of heat at its contact with the air; it is however very possible that it unites with chlorine to form hydrochloric acid.

A great many observations have in fact been recently given to the world on the presence of this acid in the vapours of Vesuvius; and, according to that excellent observer M. Breislack, it is at least as abundant in them as sulphurous acid. M. Menard de la Groye (whose conclusions on volcanos I however think too precipitate to be adopted), and M. Monticelli to whom the public is indebted for some excellent observations on Vesuvius, also regard the presence of hydrochloric acid in its vapours as incontestable. I have myself

self no longer any doubt on this fact, though during my stay in the neighbourhood of Vesuvius I could never distinguish by the smell any thing but sulphurous acid; it is, however, very possible that the extraneous substances mixed with the hydrochloric acid disguised its odour.

It is very much to be wished that M. Monticelli, who is so favourably situated for observing Mount Vesuvius, would place some water, containing a little potass, in open vessels on different parts of this volcano; the water would gradually become charged with acid vapours, and after some time it would be easy to determine their nature.

If the whole of the hydrogen furnished by water to the combustible substances contained in volcanic furnaces becomes combined with chlorine, the quantity of hydrochloric acid disengaged by volcanos ought to be enormous. It would then become a matter of surprise that the existence of this acid had not been observed sooner. Besides, the chlorine must enter into combination with the metals of silica, alumina, lime, and oxide of iron; and in order to explain the high temperature of volcanos, we must suppose that the contact of the chlorides of silicium and aluminium with water produces a great evolution of heat. Such a supposition is by no means improbable; but even if we admit it, we are still in want of a great many data, before we can render its application to volcanic phænomena satisfactory.

If the combustible metals are not in the state of chlorides, hydrochloric acid is then a secondary result; it must proceed from the action of the water upon some chloride (probably that of sodium), an action which is favoured by the mutual affinity of oxides. M. Thenard and I have already shown that if perfectly dry sea-salt and sand are both heated red hot, no hydrochloric acid is evolved: we found also that sea-salt undergoes no alteration from the agency of water alone; but if aqueous vapour is suffered to pass over a mixture of sand or of clay with sea-salt, hydrochloric acid is immediately disengaged in great abundance.

Now the production of this acid by the conjoint action of water and some oxide upon a chloride, must be very frequent in volcanos. Lava contains chlorides, since it gives them out abundantly when it comes in contact with the air. MM. Monticelli and Covelli extracted, merely by repeated washings with boiling water, more than nine per cent. of sea-salt from the lava of Vesuvius in 1822. It is exhaled through the mouths of volcanos; for very beautiful crystals of it are found in the scoria covering incandescent lava. If, therefore, lava comes in contact with water, either in the interior of the volcano,

cano, or at the surface of the earth by means of air, hydrochloric acid must necessarily be produced. Messrs. Monticelli and Covelli have in fact observed the production of acid vapours in crevices nearly incandescent; but they took them for sulphurous acid. I am, on the contrary, convinced that they were essentially composed of hydrochloric acid. It is allowable to doubt the accuracy of their observation, since they have expressed considerable uncertainty as to the nature of these acid vapours, whether they were sulphurous or muriatic.

It is well known that lava, especially when it is spongy, contains a great deal of specular iron. In 1805, on inspecting, with M. de Humboldt and M. de Buch, a gallery formed on Vesuvius by the lava of the preceding year, which after encrusting the surface had gradually sunk below it, I saw so great a quantity of specular iron, that it formed what I may be allowed to call a vein: its beautiful micaceous crystals covered the walls of this gallery, in which the temperature was still too high to permit us to stay long. Now the peroxide of iron being in a high degree fixed at a temperature much higher than that of lava, it is not probable that it was volatilized in that state: it is very probable that it was primitively in the state of chloride.

If, indeed, we take protochloride of iron which has been melted, and expose it to a dull red heat in a glass tube, and then pass over its surface a current of steam, we shall obtain a great quantity of hydrochloric acid and of hydrogen gas; and black deutoxide of iron will remain in the tube. If, instead of steam, we use dry oxygen, we shall obtain chlorine and peroxide of iron. This experiment is easily made by mixing chloride of iron with dry chlorate of potass; at a very moderate temperature chlorine disengages itself in abundance. If we suffer a stream of moist air to pass over the chloride at the temperature above mentioned, approaching to a red heat, we obtain chlorine, hydrochloric acid, and peroxide of iron. The effects observed with perchloride of iron are the same. If it be exposed to moisture, hydrochloric acid is immediately obtained, or chlorine if it be exposed to oxygen; in either case peroxide of iron is formed.

I can imagine, therefore, that iron in the state of chloride exists in the smoke exhaled by volcanos, or by their lava at its contact with the air, and that by means of heat, of water, and of the oxygen of the air, it is changed into peroxide, which collects, and assumes a crystalline form during precipitation. If we suffer a stream of chlorine at the temperature of about 400° to pass over a steel harpsichord-wire, the wire immediately becomes incandescent, but not nearly so soon as with

oxygen,

oxygen. The perchloride of iron is very volatile; it crystallizes on cooling into very small light flakes, which instantly fall into deliquescence on exposure to the air. It heats so strongly with water, that I should not be surprised, if, in a large mass, and with a proportional quantity of water, it should become incandescent. I make this observation in order to suggest to my readers, that if silicium and aluminium really existed in the bowels of the earth in the state of chloride, they might produce a much higher temperature upon coming in contact with water, since their affinity for oxygen is much greater than that of iron.

If, as can hardly be doubted, sulphurous acid be really disengaged from volcanos, it is very difficult to form an opinion of its true origin. Whence should it derive the oxygen necessary to its formation, unless it be the result of the decomposition of some sulphates by the action of heat; and of the affinity of their bases for other bodies? This opinion appears to me to be the most probable; for I cannot conceive, from what is known of the properties of sulphur, that it is an agent in volcanic fires.

Klaproth and M. Vauquelin have conjectured that the colour of basalt might be ascribed to carbon; but, to confute this supposition, we need only remark, that when a fusible mineral, even if it contain less than ten hundredths of oxide of iron, is heated to a high temperature in a crucible made of clay and pounded charcoal (*creuset brasque*), a considerable quantity of iron is produced, as Klaproth has shown in the first volume of his Essays. Messrs. Gueniveau and Berthier assert, moreover, that there remains no more than from three to four hundredths of oxide of iron in the scoriæ of highly heated furnaces. Now, as lava contains a large proportion of iron, and as the basalt which has been analysed contains from fifteen to twenty-five hundredths of the same substance, it is not probable that carbon could exist in the presence of so large a quantity of iron without reducing it*.

Is it not possible that if hydrogen be disengaged from volcanos, metallic iron, the oxides of which have the property of reducing at a high temperature, may be found in lava? It is at least certain that it does not contain iron in the state of peroxide; for lava acts powerfully on a magnetized bar, and the iron it contains appears to be at the precise degree of oxidation which alone is determinable by water; that is to say, in the state of deutoxide. I have already shown, that if hydrogen be mixed with many times its volume of aqueous vapour, it becomes incapable of reducing oxides of iron.

* When these reflections were read before the Academy of Sciences, M. Vauquelin observed that he had found carbon in the ashes ejected by the last eruption of Vesuvius.—*Ann. de Chim.* tom. xxiii. p. 195.

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The necessity which appears to me to exist for the agency of water in volcanic furnaces, the presence of some hundred parts of soda in lava, as also of sea-salt and of several other chlorides, renders it very probable that it is sea-water which most commonly penetrates into them. One objection, however, which I ought not to conceal, presents itself: namely, that it appears necessarily to follow from this supposition, that the streams of lava would escape through the same channels which had served to convey the water, since they would experience a slighter resistance in them than in those through which they are raised to the surface of the earth. It might also be expected that the elastic fluids formed in volcanic furnaces before the ascent of lava to the surface of the earth, would frequently boil up through those same channels to the surface of the sea. I am not aware that such a phænomenon has ever been observed, though it is very probable that the *mophètes*, so common in volcanic countries, are produced by these elastic fluids.

On the other hand, we may remark that the long intervals between the eruptions and the state of repose in which volcanos remain for a great number of years, seem to demonstrate that their fires become extinguished, or at least considerably deadened; the water would then penetrate gradually by its own pressure into imperceptible fissures to a great depth in the interior of the earth, and would accumulate in the vast cavities it contains. The volcanic fires would afterwards gradually revive, and the lava, after having obstructed the channels through which the water penetrated, would rise to its accustomed vent; the diameter of which must continually increase by the fusion of its coats. These are mere conjectures; but the fact is certain, that water does really exist in volcanic furnaces.

It is evident that the science of volcanos is as yet involved in much uncertainty. Although there are strong grounds for the belief that the earth contains substances in a high degree combustible, we are still in want of those precise observations which might enable us to appreciate their agency in volcanic phænomena. For this purpose an accurate knowledge of the nature of the vapours exhaled by different volcanos is requisite; for the cause which keeps them in activity being certainly the same in each, the products common to all might lead to its discovery. All other products will be accidental; that is to say, they will be the result of the action of heat upon the inert bodies in the neighbourhood of the volcanic furnace.

The great number of burning volcanos spread over the surface of the earth, and the still greater number of mineral masses which bear evident marks of their ancient volcanic origin, ought

to

to make us regard the ultimate or outermost stratum of the earth as a crust of scoriæ, beneath which exist a great many furnaces, some of which are extinguished, while others are rekindled. It is well calculated to excite surprise that the earth, which has endured through so many ages, should still preserve an intestine force sufficient to heave up mountains, overturn cities, and agitate its whole mass.

The greater number of mountains, when they arose from the heart of the earth, must have left these vast cavities, which would remain empty unless filled by water. I think, however, that De Luc and many other geologists have reasoned very erroneously on these cavities, which they imagine stretching out into long galleries, by means of which earthquakes are communicated to a distance.

An earthquake, as Dr. Young has very justly observed, is analogous to a vibration of the air. It is a very strong sonorous undulation, excited in the solid mass of the earth by some commotion which communicates itself with the same rapidity with which sound travels. The astonishing considerations in this great and terrible phænomenon are, the immense extent to which it is felt, the ravages it produces, and the potency of the cause to which it must be attributed. But sufficient attention has not been paid to the ease with which all the particles of a solid mass are agitated. The shock produced by the head of a pin at one end of a long beam causes a vibration through all its fibres, and is distinctly transmitted to an attentive ear at the other end. The motion of a carriage on the pavement shakes vast edifices, and communicates itself through considerable masses, as in the deep quarries under Paris. Is it therefore so astonishing that a violent commotion in the bowels of the earth should make it tremble in a radius of many hundreds of leagues? In conformity with the law of the transmission of motion in elastic bodies, the extreme stratum, finding no other strata to which to transmit its motion, makes an effort to detach itself from the agitated mass, in the same manner as in a row of billiard balls, the first of which is struck in the direction of contact, the last alone detaches itself and receives the motion. This is the idea I have formed of the effects of earthquakes on the surface of the globe; and I should explain their great diversity, by also taking into consideration, with M. de Humboldt, the nature of the soil and the solutions of continuity which it may contain.

In a word, earthquakes are only the propagation of a commotion through the mass of the earth, and are so far from depending on subterranean cavities, that their extent would be greater in proportion as the earth was more homogeneous.

 XVII. *Ana-*

XVII. *Analysis of a Work, entitled "Observations and Experiments made at Vesuvius during Part of the Years* 1821 *and* 1822: *by* T. Monticelli *and* N. Covelli." *By M.* Menard de la Groye*.

THE authors of this pamphlet, although both of them Italians, have thought it advisable to write in French, doubtless in order to give it greater publicity. Its form, and a great many of the expressions which we remark in it, are indeed from a paper on the same subject published in the year 1815, borrowed by M. Menard de la Groye, and inserted in the *Journal de Physique*†. It is divided into five sections or paragraphs, entitled as follows:

1°. State of Vesuvius from the time of the eruption in 1820 and 1821 up to February 1822; observations and experiments made during that interval of time.

2°. Phænomena of Vesuvius during the months of February and March 1822; and observations and experiments made during that period of time.

3°. Ascent to the crater on the 16th of March.

4°. Mineralogical and chemical examination of the products of the eruption.

5°. Last ascent to the crater on the 11th of May 1822.

These different paragraphs, composed of descriptions of forms which are known to be perpetually varying; of observations on phænomena equally subject to an infinity of changes, and of experiments which demand great details, in order to arrive at a small number of results, are, like most of the numerous papers to which Mount Vesuvius from time to time gives occasion, scarcely susceptible of a simple, clear and concise analysis. We shall therefore confine ourselves to the task of pointing out the more remarkable and certain results—those, in fact, which may be regarded as steps made in the study of that volcano which exhibits the most instructive and the most complicated phænomena of any with which we are acquainted. Forty-three articles, distinguished by the same number of figures, form the last division of the work. In quoting the observations, we shall follow the order of these numbers.

Nos. 4. and 36. The substances emitted by the volcano (*les dejections*), which cool quickly after their fall or overflow remain in a state of incoherence; but they become aggregated or agglutinated without cement wherever they are either heated

* From the *Bulletin des Annonces Scientifiques*, tom. ii. p. 435.

† Observations and Reflections upon the State and Phænomena of Vesuvius during Part of the Years 1813 and 1814, &c. Paris. V^{e} Courcier, 1815. In 4to. 102 pages, with the table of contents.

again

again from below or traversed by *fumeroles;* and our authors then perceived in the interstices of these aggregates sulphur in small octahedrons and in needles (an extremely rare kind of sublimate in this volcano), as well as sulphate of lime in silky filaments.

Nos. 5 and others. They also observe that under these circumstances the vapours exhaled abound with hydrochloric acid.

No. 9. These vapours, by attacking, on the one hand, the iron of the lavas, and by depositing in them on the other hydrochlorate of copper, have tinged the internal edges of some of the mouths with all the colours of the rainbow; and in the interior of a volcanic gulf was found a considerable deposit of snow impregnated with a small quantity of common salt.

No. 13. Pulverized nitre thrown upon the viscous paste of flowing lava does not detonate at all, and becomes liquefied without producing the slightest spark.

No. 14. This lava in a state of fusion exhibits no appearance of electricity. This fact was ascertained by every possible experiment.

No. 15. Upon plunging a tube of glass an inch in diameter, and the twelfth of an inch in thickness, into an incandescent crevice, it remained for three minutes without melting; the extremity of a bar of iron in the same situation became red-hot in five minutes.

No. 16. Distilled water in which flowing lava is extinguished contracts no acidity from it; but it dissolves a great deal of hydrochloric acid, and a little sulphuric acid in a state of combination; and some lime. It does not appear that lava rapidly cooled absorbs any atmospheric air.

Nos. 17 and 48. The most abundant smoke of burning lava has only a slight smell of hydrochlorate of iron and of copper. It did not change the colour of turnsol or violet paper either to red, or to green, and is composed of little else than aqueous vapours. The sublimed bodies do not begin to deposit themselves until the temperature of the current diminishes.

Nos. 43 and 19. It is necessary to distinguish three operations in the production or in the manifestation of the substances which are usually attributed to *fumeroles:* 1°. The local formation: 2°. The deposit of volatile substances occasioned by the diminution of the temperature: 3°. The operation of efflorescence. These three effects are explained and developed pp. 63, 64.

No. 21. Sea-salt or chloride of sodium does not appear to be completely formed in the *fumeroles;* the operation of heat

is probably only to disengage it from the lava in which it doubtless exists. Sulphurous acid is produced only when the air comes in contact with incandescent lava. The various salts which have alkaline bases, effloresce on the surface of the clods or lumps of lava and scoriæ, without the exposure of them to *fumeroles*.

No. 43, 10 and 11. Chloride of sodium is one of the salts which are found in the greatest abundance in the products of Mount Vesuvius; the masses of lava, the scoriæ, the pumice-stone, the sand, &c. are all impregnated with it: next to sea-salt, the most abundant are: 1°. the sulphates of lime, soda, potass, iron, and copper: 2°. the chloride of potassium, the hydrochlorates of iron, of copper, and perhaps of lime.

No. 22. Lava, at a red heat, does not contain acids in a free state; these acids are only developed at a lower temperature, and upon contact with the air. Hydrochlorate of iron is disengaged while the lava is at its highest temperature. There appears however to exist a large quantity of sulphur, which disengages itself even after the lava has ceased to flow.

No. 26. On one occasion when the *fumeroles* were very active, the operation of sublimation at its maximum, and that of efflorescence at its minimum, our authors perceived a powerful smell of sulphurous acid, but were unable to distinguish any smell of hydrochloric acid.

No. 29. The volcanic sand which falls upon snow contributes to preserve it from melting, by protecting it from the heat of the sun, of which it appears to be a very bad conductor.

No. 30. This sand is partly composed of very small portions of lava projected into the air in a liquid state, and suddenly cooled, and partly of solid substances pulverized by friction.

No. 31. It has the taste of sea-salt, and a sensible odour of free hydrochloric acid, which however is dissipated in a few days.

Nos. 32 and 33. The other substances which enter into its composition are, the hydrochloric, sulphuric, and silicic acids, soda, potass, lime, alumina, the oxides of iron and manganese, and a little magnesia. The predominant substances are, chloride of sodium, sulphate of lime, oxide of iron, and alumina.

No. 34. Lava, at least that which was taken from the surface, when subjected to the blowpipe, melts readily and with effervescence, and is converted into a black shining enamel. It contains 9·29 per cent. of chloride of sodium, and some traces

traces of chloride of potassium, and of sulphate of lime. Its characteristics are nearly the same as those of basalts in general.

No. 37. The heat of a current of lava continues long after all *fumeroles*, sublimations, and efflorescences, have ceased.

No. 38. A peculiar aromatic odour was perceived, which seems to indicate the presence of some new highly volatile substance.

No. 40. The best means of guarding against the suffocation occasioned by the acid vapours, is to carry with one some phials of solution of ammonia.

No. 41. It may be asserted as a general fact, that hydrochloric acid is disengaged (but not exclusively), and that sulphur becomes apparent when the temperature of the volcano is below ignition, and that sulphurous acid can only be formed by contact with the air and at a higher temperature.

Some of these facts are found quoted in the *Récapitulation des faits observés*, which is given under the forty-third and last number. The other facts noticed there were already more or less known.

In the memoir of Messrs. Monticelli and Covelli we must notice with particular approbation the chemical experiments with which it abounds. They are given in detail, and accord with the present state of the science. This part of the study of Mount Vesuvius, and of all other burning volcanos, had been, until the present time, almost entirely neglected.

XVIII. *Notice on the " Essays of* JEAN REY." *By Mr.* JOHN MURRAY, *F.L.S. M.W.S.*

PROF. BRANDE in his eloquent and able dissertation on Historical Chemistry, in the Supplement of the Encyclopædia Britannica, has alluded to the extreme rarity of the work of Jean Rey, even of the reprint of 1777.

We are indebted to that distinguished chemist J. G. Children, Esq. for a masterly translation of the copy (edition 1777) of these Essays (in the Library of the British Museum), and published in the Quarterly Journal of the Royal Institution.

It may not be uninteresting to mention, that I purchased at Paris in February 1819 a copy of the FIRST edition of this celebrated and interesting work at the sale " de la bibliotheque de M. P***." It was marked in the catalogue " très rare de cette edition."

Allow me now to quote what I find written on the blank leaf preceding the title page. It is scarcely possible in perusing these

these remarks to believe that Lavoisier was unacquainted with a volume which was so well known to others, and referred to in their writings.

"Cet ouvrage, écrit dans le style de Montaigne, mais avec plus de methode et moins de diffusion, a été reimprimé en 1777 à Paris, avec des notes de M. Gabet.

"Il était devenu extremement rare, et la reimpression en été faite suivant l'exemplaire de la Bibliotheque du Roi.

"Spiedman, professeur de chymie à Strasbourg, en recommande la lecture à ses éleves dans son institution de chymie en 1766.

"M. Bardeu en fait une mention honorable dans ses Recherches sur les maladies chimiques—même éloge dans la Mineralogie docimatique de M. Sage, &c.

"Cette edition est d'autant plus remarquable, que l'on n'avait presqu'aucun ouvrage imprimé à Bazas, petit trou dans le Gascoigne, où jamais il n'y a eu ni litterateurs, ni aucune autre imprimerie que celle de Guillaume Millanges, qui n'y a fait qu'un court sejour."

Next to the "Tractatus de Respiratione" of John Mayow of Oxford, "Lugd. Batavor 1671," the "Prælectiones Chymicæ" of Freind, of which a translation was published at London in 1729, have interested me. In this last the attractive forces of chemical combinations;—the weight acquired by "calcination;"—"fermentation" as "raised by elastic particles;"—the determinate forms of crystals—are all taught in a masterly manner. I am yours, &c.

Stranraer, N.B., July 10, 1823. J. MURRAY.

XIX. *An Account of the Observations and Experiments on the Temperature of Mines, which have recently been made in Cornwall, and the North of England; comprising the Substance of various Papers on the Subject lately published in the Transactions of the Royal Geological Society of Cornwall, and other Works.*

[Concluded from p. 46.]

V. THE concluding paper on the Temperature of Mines, in the Transactions of the Cornish Geological Society (vol. ii. p. 404), is by Mr. Moyle, who has since detailed the substance of it, with some additional facts, and remarks on the statements of Mr. Fox and Dr. Forbes, in the Annals of Philosophy for January last, p. 43; still more recently, also, in the same journal, for July, p. 15, he has given some further results as to the

the temperatures of three mines mentioned in his first memoir. As each communication principally relates to the phænomena of the same mines, it will be most convenient and useful to give the substance of them all under one head, in a connected form.

"So far back as the year 1812," Mr. Moyle informs us in his first paper, "my attention was drawn to the consideration of the temperature of our mines, from the circumstance of meeting with water in different levels which felt of various degrees of heat. From this time I began to collect notes of the temperature of several mines; but as they were made merely for my own gratification, without any idea of their meeting the public eye, I do not feel sufficient confidence in them to state that they are entirely free from error, as it is possible that many adventitious circumstances were disregarded, which might have affected the result. I shall, however, select only a few of my early experiments, making a distinction between them and those made within the last twelve months, which were made with all possible care and exactness. This I have considered more particularly necessary, as the subject has become of late a matter of serious inquiry, and various opinions have been formed as to the relative temperature of the interior strata of our earth, and its causes."

"As it is only by comparing the different results of the experiments of individuals that the truth, or an approximation to it, can be elicited, I conceive too much attention cannot be paid to the mode in which those experiments are conducted. With respect to the temperatures now given, where there has been any degree of uncertainty in the result, they have been taken twice or thrice in the same spot, by different methods, such as burying the thermometer in the earth, or rock of the gallery,—in the full stream of water issuing from the veins,—by allowing it to remain 15 or 20 minutes during each observation,—and by the correspondence of two or three thermometers at the same time."

Mr. Moyle now details his experiments, the results and circumstances of which we have arranged into the following table, appending to it, in remarks and notes, such particulars as were found unsusceptible of tabular abbreviation: into this table, likewise, we have introduced some facts given by the same gentleman in his second paper.

Tem-

Depths from the surface, in fathoms.	Temperature of Mines at work.							Temperature of the water, in mines which have long been abandoned.									
	Huel Unity, temp. at surface in shade 67°.	Creegbraws copper mine, on same lode as H. Unity, but considerably elevated above it; temp. at surface 43°.	Huel Trumpet tin mine, in granite; temp. at surface, Jan. 1822, 58°: do. of A. w. 56°.	Trenoweth copper mine; temp. at surface, April 29, 1822, 63°.	Oatfield copper mine, on same lode as Trenoweth; temp. in E.S. at adit-level, betw. 25 & 34 fath. deep, May 1822, 61°.	Crenver copper mine, on same lode.	Huel Abraham, on same lode.	Trevenen tin mine; temp. at surface, 56°.	Herland copper mine, in Gwinear; temp. at surface, May 28, 1822, in shade 64°, in sun 74°.	Huel Pool lead mine, near Helston; June 8, 1822.	Huel Rose, ditto; same time.	Huel Alfred; July 1822.	Relistian, in Gwinear; same time.	Huel Ann, tin mine, in granite, on same lode as Huel Trumpet.	Huel Franchise, tin and copper lode; parallel with H. Trumpet.	Huel Nancy, on same lode, temp. at surface, in shade, 55°.
9	E.S. 74 *															200 feet deep; temperature at all depths 51°.
10	......	51	......	......	......	......	....	....	......	53	E.S. 53½					
14	......	......	......	......	......	......	....	A.w.52††								
18	......	......	......	......	......	......	....	....	......	..	..	A.56		52		
19	......	......	......	......	......	......	....	....	......	..	..	..	..			
20	......	R. 55 W. 60	......	......	......	......	....	....	......	53§§	E.S. 53½					
23	R. 54 (10)															
24														52		
25	......	......	......	......	......	......	....	....	......	..	..	..	..			
28	......	......	......	......	......	......	....	....	......	..	..	..	A.55			
30	......	54 62	......	......	......	......	....	....	......	..	..	56		..		
32	......	......	......	A.w. 54 56 †	......	......	....	....	......	..	..	..	..			
34	......	......	......	......	......	......	....	....	A 52 53 ‡‡ S.(60nw)54	..	..	..	..	52	51	
35	......	......	......	......	......	......	....	....								
40	......	......	......	......	R. w. 56 W. w. 61	......	....	....	......	..	..	..	55			
42	......	......	......	......	......	......	....	....	......	..	E.S. 53¼					
44	......	......	R. 54 W. 58	......	......	......	....	....	E.S. 54 S.(60nw)56							
45	......	......	......	E.S. e. 54 E.S. w. 54 53 (40)	......	......	....	....	......	..	..	56				
47	......	......	......		......	......	....	....	......	..	..	..	55			
50	R. w. 54½ W. 62 W. 63½	......	......	......	R. w. 56 W. w. 58	R. 55 (50 e)	....	....	......	..	..	..	55			

52	......	......	......	e. 54½	......	......	....
54	......	......	......	......	......	......	....
58	......	......	......	......	......	......	....
66	......	......	......	w. e. } 54‡			
70	......	......	......	......	W { e. 60 w.56 }		
72	......	......	{ R. w.52 W. w.67 }	......	......	R. 56	....
75	......	......					
78	......	......	......	......	......	......	....
82	......	......	......	......	......	R. 61	
86	......	......	{ R.w. 53 W. 68½ }				
92	......	......	......	......	......	......	....
102	......	......	......	......	......	R. 62	
112	......	......	......	......	......	R. 64	
118	......	......	......	......	......	......	....
122	......	......	......	......	......	{ E.S. 64 R(60 e.)60 }	
120	......	......	......	......	......	......	....
132	......	......	......	......	......	{ E.S. 63 R(200 e.)61 }	....
134	......	......	......	......	......	......	....
142	......	......	......	......	......	{ E.S. 62 R.(30w)64¶ W.(200e)68 }	
150	{ R. 65 W. 74 }						
182	......	......	......	......	E.S. 77		
212	......	......	......	......	78 (80 e.)		
222	......	......	......	......	{ R.81(100 e.) R.78 (60 w.)§ W. 80(12 e.) }	......	W. 84
232	......	......	......	......		......	W { 90 86 } **
236	......	......	......	......	{ 85 w. 82 w86½ } (30w)‖		
242	......	......	......	......	......	......	{ W. 84 R. 86 }

....	{ E.S. 54 S. (60 nw.)56 }					
....	......	..	E.S.53‖‖	..	..	52
....	......	..	..	56		
....	{ E.S. 54 S. (60 nw.) 56 }					
....	......	..	..	56		
....	E. S. 54					
....	......	..	..	56		
....	......	..	..	56		
....	E. S. 54					
....	......	..	..	..		52¶¶

Remarks.—Many of the workings in *Creegbraws* are very extensive, bu most of them have been long since relinquished.

Trenoweth had been discontinued for more than twelve months from the dat of the paper (Oct. 1822) so far as regarded the presence of workmen but the engine was still kept at work, for the purpose of assisting the en gines of *Crenver*.

In *Crenver*, very little work has been done for a great while.

Huel Abraham is in full work in almost every part.

Herland has ceased working, except above the adit, for 15 years.

Relistian is reported to have been, when at work, much hotter than mine in general.——*Huel Ann* has not been wrought for the last 20 years, bu is to be immediately re-worked (Dec. 1822).——*Huel Franchise* ha ceased working for about two years (Dec. 1822).——*Huel Nancy* ha stopped more than 20 years.

Abbreviations employed in the preceding Table:

a. air; *e.* earth, including the rock of the walls and floors of the galleries; *w.* water; A. adit; S, shaft; E. S. engine-shaft; R relinquished part of the mine; W working part, the figures between parentheses, as (10), denote the distance from the engine-shaft, in fathoms, of the part of the mine at which the temperature was taken, and this either in an east (e.) west (w.) or north-west (nw.) direction.

Notes to the preceding Table.

* "At the depth of nine fathoms in the engine shaft, the steam arising was so dense that our candles were of little use: it had the temperature of 74°, and was extremely oppressive, which induced us to descend by another route."

† "The adit is 32 fathoms deep: the water here (at the distance of 260 fathoms from the mouth of the pump by which it is drawn up) was 54°; but it gradually increased in temperature from this place to the mouth of the pump, where it was 56°."

‡ "To show the influence of a few (three) persons on the temperature of the air of a small mine, I found the air in the adit, on our return, 1° warmer than at our descent."

§ "Neither of these spots were working places, but the latter was more contiguous to them than the others."

|| This was in a confined *end;* here the water issuing from two small veins, a few feet apart, indicated the above temperatures.

"Since the temperature of the different parts of this mine [Oatfield] has been taken, the pumps have been drawn up from the deepest part, and the shaft, below the depth of 182 fathoms, has been for some months full of water. At this level the temperature was previously 77°; but a few months afterward (Sept. 1822), when the water had risen to the level, its temperature, a few feet below its surface, was 69°; and at the depth of 12 feet in the water, it was 71°. A fortnight after this I repeated the experiment, and found the temperature, a few feet below the level, 66°; and at 12 feet deep in the water 67°. This shows that the water is gradually cooling, and becoming of the temperature of the surrounding earthy strata, it having cooled 3° in a fortnight, and 11° since its admission into the shaft."

¶ "I have since ascertained the temperature of three levels which have been driven from Crenver, directly under the deepest level in Trenoweth. At the depth of 124 fathoms [below the adit-level] it was 57°,—at 132 fathoms, 58°,—at 142 fathoms 58°. Five months before, when the miners were at work in the last-mentioned level, the temperature was 68°. To what then must we attribute that superiority of temperature?"

** "At 232 fathoms deep, at the extremity of the level, on a Monday morning, before the workmen had returned to labour, and where a machine was erected for blowing fresh air to the miners, the thermometer stood at 90°; but a few days afterwards, when a communication had been opened, it fell to 86°."

†† The temperature of the water in one of the shafts, which reached to the lowest part of the mine, and from which it continually overflowed, was 52½°.

‡‡ The temperature of the water running through the adit, was 52°; "as we approached the engine-shaft, it was increased to 53°."

§§ In another shaft, the temperatures were precisely the same, at the same depths.

|||| "The time allowed for the thermometer to remain at the different depths (except the last) was ten minutes, which perhaps was scarcely long enough."

¶¶ This was the bottom of the mine, where the thermometer was allowed to remain for four hours.

"In

"In making a few observations on the foregoing experiments," Mr. Moyle continues, "I must remark, in the first place that in mines which are at work scarcely two places of equal depth below the surface, and under similar circumstances, exhibit the same temperature; nor can I find, where it increases with the depth, that there is any certain ratio; it being often, as in Oatfield, colder at 70 than at 40 fathoms deep. As these differences and irregularities of temperature always occur in mines which are at work, they must arise from adventitious causes. I am therefore of opinion that the true temperature of any part of a mine in the full course of working, is difficult of attainment, and that we must have recourse to those mines, and parts of mines, which have been long since quitted by the miner, in order to obtain any thing like a true datum."

"Most of the deep mines now at work show an increase of temperature of more than 1° for every 10 fathoms of descent; at least it amounts to this on the whole, although its progressive ratio is not in that proportion: to show therefore that this increase of heat arises from causes operating only in mines at work, we have merely to refer to the temperature of mines long since relinquished, the highest of which appears to be 56°, only 3° above the mean of this neighbourhood."

"The experiments already mentioned clearly prove that the water in relinquished mines exhibits nearly the same degree of temperature at all depths; and as it is demonstrated that water is a bad conductor of caloric, except in an upward direction, it is natural to infer that the temperature of the deepest parts of those mines which are full of water, may be ascertained by sinking the thermometer a few feet or fathoms below the surface."

"The water in the two shafts of Herland Mine indicated different temperatures; viz. 54° and 56°, and this difference was the same at all depths. Now had the temperature of the earth been uniform at the same depth, there would have been no difference whatever."

"The temperature of the water at the deepest part of Huel Alfred (130 fathoms) was only 56°; but according to the theory of progressive heat, it ought to have been 66°."

"The hot springs which frequently occur, whilst they prove the existence of causes sufficient to give them their high degree of temperature, prove at the same time, by their rarity, the local and adventitious nature of those causes. Such springs are sometimes met with at the very bottom of our mines, as at Dolcoath."

"The water issuing immediately from copper veins is generally

nerally warmer than that which flows from those of tin. May not this arise from the action of oxides of iron and sulphuretted pyrites, which are more abundantly found in copper than in tin lodes?"

"I need not here enumerate the many adventitious causes of heat in mines; but from the whole of my experiments I cannot but conclude that the doctrine of the progressive increase of heat in proportion to the depth, is without foundation, since it has been proved that it may be as cold at the depth of 100 fathoms as the mean of this climate, which could not be the case were the progressive theory correct."—(*Corn. Geol. Trans.* vol. ii.)

"On repeating my experiments on the temperature of the water in Herland mine, I found the heat at all depths, as before stated (see the table), viz. 54° in the old engine-shaft, and 56° in another about 60 fathoms distant; and in a third, not before tried, the water was only 52°. I was given to understand by Capt. S. Grose, who accompanied me, that all these shafts extended to nearly the same depth. This circumstance I conceive rather remarkable, and clearly proves the operation of different causes of temperature in a very circumscribed portion of ground."—(*Ann. Phil.* Jan.)

"Many of the experiments" in Huel Abraham, Crenver, and Oatfield copper mines, "were a few days since [in last May] repeated in precisely the same spot, and under similar circumstances as before, and nearly with the same results, excepting the temperature of the water accumulated at the bottom of Oatfield engine-shaft below the depth of 182 fathoms from the surface, in consequence of the pumps being drawn up from below that level."

"The coldest part of this water ten months ago, at the depth of 1,164 feet from the surface, and in 12 fathoms of water, was 66°. Last week, at precisely the same depth, it was only 59°; while the water at the surface of this shaft was 77°. This increase of temperature at the surface is to be attributed to the immense quantity of warm water sent from distant parts of the other mines to this shaft to be drawn out, and although it falls several feet into this shaft, which keeps the water in a constant and great agitation, yet it does not affect the temperature at the above-mentioned depth so much as might be expected."

"I regret much that the registering thermometer could not be sunk to a much greater depth, and quite out of the influence of the falling waters, as I am inclined to think that it must ere this have arrived, or nearly so, to the mean annual temperature, or 53°."

"I have

"I have before shown that by admitting the gradual increase of temperature (according to our descent) after a certain ratio, the temperature of this depth ought to be at the lowest calculation 70°. How comes it then to be less by 11° and 18° *minus*, since this place was in the full course of working?"

"I have also found that the temperature of a working spot in Huel Abraham at the 180 fathom level, where the difference of atmospheric pressure was 0·964, or nearly one inch, when other circumstances, such as number of men, current, blasting of rocks, &c. &c., were similar, that the difference of temperature was only from $1\frac{1}{2}$° to 2°; it being 78° when the thermometer was lowest, and $79\frac{1}{2}$° to 80° when highest."—(*Ann. Phil.* July.)

"In making my experiments with the registering thermometer, in order to obtain as correct results as possible, I always reduce the degree of the mercurial one to about the freezing point, by sprinkling its bulb with ether, and by raising the spirit one with my tongue, bringing the indices to correspond before each immersion."

"There appears to be little or no difference in the mean temperature of the same spot in a *deep* and confined part of a mine at work, in summer or winter; or at least the miners are not sensible of any. Capt. W. Teague assures me, that he has often met with ice in great abundance in Tin-Croft mine, at the depth of 318 feet below the surface, and in such quantities that the ladders have been impassable; deep crevices in the walls have been completely filled, and icicles hanging abundantly around him*.

After next explaining, on the well known principles (in the *Ann. Phil.* for Jan.) how the whole body of water in a relinquished mine becomes of an uniform temperature, and citing certain observations of Mairan, Hales, Marriotte, and Van Swinden, respecting the alternate heating and cooling of the earth's surface in summer and winter, and the nearly equable temperature

* To this statement by Mr. Moyle, we subjoin an extract relative to the formation of ice in mines, from the concluding volume of the late Dr. E. D. Clarke's Travels, which has lately appeared. He is describing a descent into one of the great iron-mines of Persberg, near Onshytta, in Sweden: the depth of the mine from the rocky surface to where the buckets of ore were filled appears to have been about 75 fathoms.

"As we descended further from the surface, large masses of ice appeared, covering the sides of the precipices. Ice is raised in the buckets with the ore and rubble of the mine: it has also accumulated in such quantity in some of the lower chambers, that there are places where it is fifteen fathoms thick, and no change of temperature above prevents its increase. This seems to militate against a notion now becoming prevalent, that the temperature of the air in mines increases directly as the depth from the surface, owing to the increasing temperature of the earth under the same circumstances

temperature of the earth at all seasons, at small distances below its surface, Mr. Moyle gives a table of the temperatures of mines, containing the results he had before communicated to the Society; and of which we have given a tabular arrangement at p. 96, incorporating with them the temperatures of several other mines, additionally presented in Mr. Moyle's table. He then proceeds to the following remarks on what Mr. Fox and Dr. Forbes have stated, as to the progressive increase of heat in the earth, in proportion to the depth.

"In Mr. Fox's tables, the irregular ratio of augmented temperature is very conspicuous; as it appears to be as hot at the depth of 600 feet in Chacewater mine, as it was in Dolcoath at the depth of 1440 feet, each being 82°. In the next place, it is as hot at 420 feet in the United Mines, as in Dolcoath at 1200 feet; as hot in Chacewater at 480 as at 840 feet in Huel Damsel; as hot at 780 feet in Treskerby as at 1380 in Dolcoath, &c. &c., and hotter in the United Mines at the depth of 1080 feet than in any other mine in the county. From this statement, it appears that the temperature of the earth in Chacewater increases 27° in 540 feet in depth; while Dolcoath is augmented only the same in 1380 feet; and the United Mines the same number of degrees in 1080 feet, or exactly double the depth. These facts would induce me to look upon the progressive ratio of heat in a different light from those gentlemen."

"Mr. Fox and Dr. Forbes are at variance in opinion about

stances and in the same ratio; but it is explained by the width of this aperture [a natural cavernous fissure] at the mouth of the mine, which admits a free passage of atmospheric air. In our *Cornish* mines, ice would not be preserved in a solid state at any considerable depth from the surface."—p. 103.

In Dr. Clarke's account of the Fahlun copper-mine, at p. 141 of the same volume, after stating the descent of himself and companions to the depth of 170 fathoms, he observes, "Here we found the heat very oppressive: the miners, with the exception of their drawers and shoes, were naked at their work. This high temperature, increasing always in the direct proportion of the descent from the surface of the earth, and which may be observed in all mines, has never been satisfactorily explained. In the great mine of *Poldice*, near *Truro* in *Cornwall*, which has been worked, in *granite*, to the depth of 300 fathoms, the miners, as at *Fahlun*, carry on their labours naked; and the heat is so great at the bottom of the mine, notwithstanding the accumulating water, that it may be sensibly felt by any person placing his hand against the sides of the rock, as the author himself experienced. The heat of the *Fahlun mine* is so great, that it becomes intolerable to a stranger who has not undergone the proper degree of *seasoning* which enables a miner to sustain it. But then there are causes which tend greatly to increase the natural temperature: prodigious fires are frequently kindled, and at a very considerable depth in the mine, for the purpose of softening the rocks previously to the application of gunpowder: add to this, the terrible combustion [of pyritous matter] which has taken place in the mine, threatening its destruction."

fixing

fixing a limit as to the precise point below the surface, for the commencement of augmented temperature: an examination of an experiment or two will prove the confidence we may place in the conclusions of either."

"Mr. Fox commences at 50 feet, and Dr. Forbes at 200 feet below the surface; and from the extreme temperature observed in our deepest mines, would deduct 6° for artificial and extraneous causes of heat, thus reducing the actual degree at about 1300 or 1400 feet to from 72° to 74°*; and after the ratio of 1° for every 50 feet, it would be at the depth of 1044 feet, 68°. Now reverse the order of calculation, and we shall find Mr. Fox to make it 69½°, and Dr. Forbes 66½° for the same depth. This is the precise depth of the lowest of the three levels driven under Trenoweth from Crenver, the temperature of which is actually only 58°, although a spot not in the course of working, yet has a distant communication with the mine in general, and at a working spot on the same level, the temperature is but 68°, after being exposed to all the extraneous sources in common."

Mr. Moyle next quotes an observation of Dr. Forbes, respecting the evidence of the natural high temperature of the Cornish mines, which is afforded by that of extensive collections of water in abandoned mines and workings (for which see our last vol. p. 446), and then inquires, "Will these gentlemen still maintain the same sentiments? If so, their theory must fall to the ground, as we can now clearly prove that these very collections of water possess even a less temperature than the supposed mean of the climate; *e. g.* Huel Ann, and the third shaft in Herland; one 130, and the other 160 fathoms in depth, Ding-Dong, Huel Rose, Huel Franchise, &c."

* Taking, then, the mean results of my observations on the different mines, as given in the last column of the table, it will be found that the mean rate of increase is about one degree for every 50 or 60 feet."

"This result comes very near that drawn from the observations of Mr. Bald in the coal mines (see p. 105), and agrees with the deductions of my friend Mr. Fox, which have been presented to the Society. Admitting this as the true result of our observations, I should be disposed to deduct six or seven degrees from the extreme amount in our deepest mines, as attributable to the artificial and extraneous causes formerly detailed, and would thus fix the actual temperature of the solid matter of our earth (in Cornwall) at the depth of from 1300 to 1400 feet, at from 72° to 74° of Fahrenheit."—*Dr. Forbes, Trans. Corn. Geol. Soc.* vol. ii. p. 208.

"At what precise point below the surface the augmentation of temperature commences, I am unable to say with any degree of confidence; but from a consideration of the influence of extraneous causes in modifying the temperature observed in the superior galleries of mines, and from some particular observations made by myself, I am disposed to place this point at about the depth of 200 feet from the surface."—*Ibid.* p. 210.

He

He then examines what he considers to be the only important instances of the above kind, which Dr. F. brings forward in support of his conclusions, viz. those of the collections of water in the mines of Botallack and Little Bounds; observing, with respect to the first, that "the heat of *this water at the bottom of the working is not given*;" and asking, as to the second, the temperature of which, Dr. Forbes states, "as discharged by the pumps in 1822, is $56\frac{1}{2}$.°"—"Pray what has this to do with the temperature of the central part or bottom of the collection?"—"And yet Dr. F.," continues Mr. M., "in nearly the following page, states, that a large body of water resembling the last has accumulated in the old wrought part of Ding-Dong mine; at the depth of 444 feet below the surface, the workmen had just cut through the barrier which divided them from this old working, and the stream of water which issued forth (and which was the bottom of the large collection) was only $52\frac{1}{2}$, thus at once proving what is actually the case, that, as I before stated, it may be as cold at the very centre of the earth as at any distance beneath its surface."

"In the next place, I do not conceive that their opinion can be supported, because Dr. Forbes's philosophical reasoning on all the extraneous sources of caloric falls short of what is actually observed, and that we must attribute this extra portion as derived from the earth itself; for I should imagine that there are few more difficult problems, than a true estimation of the power of the *infinite* sources of caloric in a mine in the full course of working."—*Ann. Phil.* Jan. p. 37–43.

VI. Since the present article was prepared for the press, the following "*Notice in regard to the Temperature of Mines*," by Mathew Miller, Esq. M.W.S. has appeared in the Memoirs of the Wernerian Natural History Society, vol. iv. part ii. p. 466.

"The late experiments on the temperature of mines made in Cornwall, and in other countries, having given rise to various speculations in regard to the distribution of heat in the crust of the earth, all of which appear to me to be unsatisfactory, I now beg leave to offer for consideration of the Society, an explanation, that does not seem liable to the objections that have been opposed to the others.

"In every mine, with the exception of a few, which are level-free, the ventilation is carried on by causing the air at the surface to descend, and traverse the works, and then ascend. Now, it is evident, that if a portion of air from the surface be carried down to the bottom of the mine, it will be condensed in proportion to the depth of the mine, and, in consequence of this condensation, will become heated, and the degree of heat will

will of course be in proportion to the depth of the mine. The air thus heated traverses the works, and imparts its heat to the strata; it then ascends, and is succeeded by a fresh portion of air from the surface, which in the same way becomes heated, and imparts its heat to the strata, and they, in turn, communicate it all around. Thus in a long course of working in a deep mine, the air at the bottom is heated, and also the rocks to a considerable depth; and when the working ceases, the mine takes a long time to lose its temperature; and this is found to be the case particularly when the mine becomes full of water, the water being found at first of a high temperature, and gradually to lose its heat, which is in consequence of the strata imparting theirs to the water; and as soon as they have given out all their heat, the water indicates the mean temperature nearly of the place.

"The reverse takes place in an old mine when reworked; in that case, the temperature rises gradually as the working continues: and in those mines which are not worked, but in which the ventilation still goes on, I believe it will be found that they do not lose more of their temperature than can be placed to the abstraction of the other causes of heat in working mines, such as that produced by the men and the lights.

"The exact quantity of heat given out by air in proportion to its condensation, it is difficult to ascertain; but every day's experience proves it to be very considerable; and, I believe, this, added to the other obvious sources of heat in mines in a state of working, will be found sufficient to account for their high temperature."

We will now close this article, for the extent of which the great importance of the subject must be our apology, with a brief abstract of a paper "*On the Temperature of Air and of Water in the Coal Mines of Great Britain;*" by Mr. Robert Bald, F.R.S. E. &c. (Read before the Royal Society of Edinburgh in 1819) as given in the Edinburgh Philosophical Journal, vol. i. p. 134.

"The increase of temperature in coal mines, is a fact familiar to every person who has had occasion to frequent them. The instant a dip-pit is connected with a rise-pit by a mine, a strong circulation of air like wind commences. If the air at the surface is at the freezing point, it descends the dip or deepest pit, freezes all the water upon the sides of the pit, and even forms icicles upon the roof of the coal within the mine; but the same air, in its passage through the mines to the rise-pit, which is generally of less depth, has its temperature greatly increased, and issues from the pit-mouth in the form of a dense misty cloud, formed by the condensation of the natural vapour of the mine in the freezing atmosphere."

"The following table presents at one view the temperature of air and water in the deepest coal-mines in Great Britain."

	TEMPERATURES.													
Depths.	Whitehaven, Cumberland.		Workington, Cumberland.		Teem, Durham.		Percy Main, Northumberland.		Jarrow, Durham.		Killingworth, Northumberland.		Prince's-end Pit, Staffordshire.	
	air.	*water.*	*a.*	*w.*	*a.*	*w.*	*a.*	*w.*	*a.*	*w.*	*a.*	*w.*	*a.*	*w.*
Surface	55	49*	56	48*	...	...	42	...	49½	...	48	...	...	...
180 feet	...	...	...	50	...	...	...	...	...	...	...	...	...	...
above 400	...	...	...	...	...	...	...	...	...	...	...	...	...	47½†
444‡	...	...	...	...	68	61	...	...	...	...	...	...	...	...
480	63	60	...	...	...	...	...	...	...	...	...	...	...	...
504§	...	...	...	60	...	...	...	...	...	...	...	...	...	...
600	66	...	...	...	...	...	...	...	...	...	...	...	...	...
790	...	...	...	...	...	...	...	...	...	...	51	...	...	...
882	...	...	...	...	...	...	...	...	70	68	...	...	...	...
900	...	...	...	...	...	...	70‖	68	64¶	...	70**	...	...	...
1200	...	...	..	...	...	...	...	...	...	...	77	74††	...	...

"It has been found from experience, that the deeper we perforate the strata, they become drier, and in many instances no water is to be found, so that the roads through the mines require to be watered, in order to prevent the horse-drivers from being annoyed by the dust; and there is reason to believe, that the high temperature of the air in Prince's-end pit, was occasioned by the decomposition of pyrites amongst the rubbish of the coal, which frequently produces actual and vehement combustion. The increase of temperature, as given in the preceding experiments, appears to have its origin in a constant natural internal heat in the physical constitution of the earth."

"It has been asserted by those who have considered the temperature of mines that the heat found there arises from the workmen, and from the lights and horses employed in the mines. These causes, however, cannot produce more than a degree or two of temperature, as the air is necessarily kept in constant circulation for the safety of the workmen."

* Springs. † "Air in the mines 60°." ‡ "In a country a little elevated above the sea." § Estimated from the level of the ocean, "and beneath the waters of the Irish Sea."

‖ The depth reckoned from the level of the sea, "and under the bed of the river Tyne, ...at this depth Leslie's hygrometer indicated dryness 83°."

¶ "Air at the pit bottom 64°;......the engine pit of Jarrow is the deepest perpendicular shaft in Great Britain, being 900 feet to the foot of the pumps."

** After the air has "traversed 1¼ mile from the bottom of the downcast pit." †† "At the most distant forehead or mine.......This mine is the deepest coal mine in Great Britain;.....in this mine the temperature of distilled water at the boiling point was 213°; the temperature of the same water at the surface 210½°."

"Others

"Others have asserted, that the increased temperature arises from the decomposition of pyrites, which abounds in coal and the accompanying strata, and that this is the cause of the high temperature of hot springs. This opinion, however, does not seem to be well founded. Although in the very extensive coal-mines of Great Britain, pyrites abound in great quantities; yet in no instance was pyrites ever found *decomposed in situ*, although the coal abounds with water, and gives out carbonic acid gas and carburetted hydrogen, but never atmospheric air, and the pyrites only decomposes when exposed to the action of oxygen. Had pyrites been liable to decompose *in situ*, the greater part of the coal-fields in the world would have been destroyed by spontaneous ignition; but this spontaneous ignition only takes place in coal-mines where the pyrites is thrown into the waste, and exposed to the action of atmospheric air, and the moisture of the strata. If pyrites is the cause of the high temperature of hot springs, these springs would vary continually, both in temperature and composition, according to the extent of surface exposed to the decomposing action." (E.)

XX. *On Electro-Magnetism. By Mr.* J. Tatum.

To the Editors of the Philosophical Magazine and Journal.

I EMBRACE this opportunity of sending you part of a second paper, read before the City Philosophical Society, on Electro-magnetism.

It will be recollected, that in my former paper I observed that there were eight parts on each side of the equator of the needle which appeared to be attracted and repelled by the connecting wire of the Voltaic apparatus.

I also closed that paper by showing, that these attractions and repulsions were the reverse on that part of the wire connected with the zinc (or positive) side of the apparatus, to what they were with the part connected with the copper (or negative) side.

I next wished to ascertain if it were possible to demonstrate an uniform direction of any particular part of the needle round the positive and negative wires.

Exp. 1. For this purpose I brought the tip of one side of the north end of a dipping needle (on one end of which was attached a piece of copper wire to counteract its dip) parallel with, and near the *left* side of, the negative wire DC, (fig. 13 of the last Number of the Phil. Mag.) when it *descended.*

Exp. 2. I then brought it *under* the wire, and it turned to the *right.*

Exp. 3. It was next placed near the right side of the wire, when it *ascended.*

 Exp.

Exp. 4. When removed to the *upper* side of the wire, it rotated to the *left.*

These movements of the needle may be represented by Fig. 1, in which A may represent a section of the horizontal negative wire, and the heads of the arrows the direction in which the needle rotated.

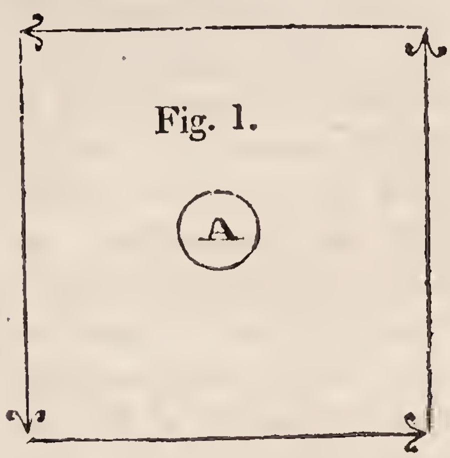

Fig. 1.

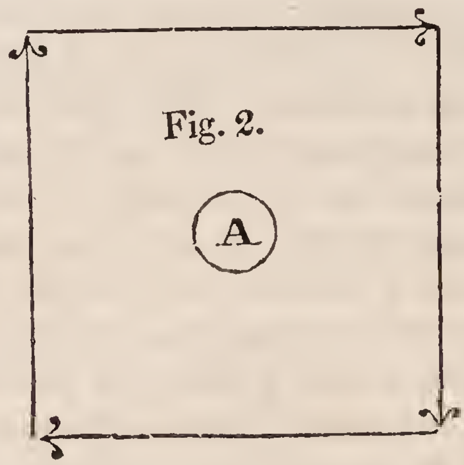

Fig. 2.

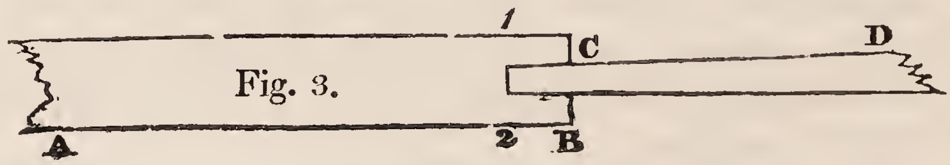

Fig. 3.

I now wished to see what effects would be produced on the needle by the positive wire; for which purpose,

Exp. 5, It was brought near the *left* side of GH, Fig. 13 (see last Number), in which situation it *ascended.*

Exp. 6. It was then removed to the under part of the wire, when it turned to the *left.*

Exp. 7. It was next placed on the *right* side: here it *descended.*

Exp. 8. And finally, when it was brought *above* the wire, it passed off to the *right:* which may be represented by Fig. 2, in which it will be seen that the motions of the needle at the positive wire are the reverse of those at the negative wire: and if the south end of the needle be made use of, all the above effects are reversed; from which it must appear, that there was an evident tendency of the needle to rotate round the positive and negative wires, but in opposite directions.

Exp. 9. Having shown, in a lecture which I delivered before the above Society, that when an electrical discharge of a battery of five jars, equal to twenty-one coated feet, was made to traverse a helix which was coiled from left to right, and which contained pieces of steel, it not only communicated to them magnetic properties, but their north poles were directed towards the *positive* side of the battery.

Exp. 10. But when a helix coiling in a different direction was used, the north poles of the pieces of steel were directed towards the *negative* side of the battery.

I was desirous of showing the analogous effects of common and Voltaic electricity, not only in communicating magnetism to

to pieces of steel, but as respects the direction of their poles; for which purpose,

Exp. 11, I inclosed a piece of steel in a helix similar to *Exp.* 9, which connected the copper and zinc side of the apparatus (described in your last number): in a few moments it became magnetic, and its north pole was towards the zinc or positive side of the apparatus.

Exp. 12. Another piece of steel was inclosed in a helix coiled from right to left. After a few moments it was examined, when its north pole was found to be towards the copper or negative side of the apparatus: so that it appears evident that the *poles* of the steel, rendered magnetic by either common or Voltaic electricity, are determined by the direction of the coils of the helices.

I cannot help noticing the similarity which appears to exist between the direction in which it is necessary for the electrical current to traverse, in order to render ferruginous bodies magnetic, and the *direction* in which I *suppose* the magnetic influence traverses in magnetic bodies.

I am aware that pieces of steel *may* have been rendered magnetic by passing an electrical charge across them; but I have never produced such *powerful* magnets by this means as by the use of the helix; indeed the experiment has been rather uncertain, but after all it is but an imperfect modification of the helix.

I am fully sensible that an erroneous theory may be advanced to explain the phænomena of experiments, and I am not so partial to my opinion as to *insist* that I may not labour under some mistaken idea; but I cannot conceive how those movements are produced on the dipping needle, rotatory apparatus, &c., if the magnetic fluid passes in *straight* lines from one part of the needle to the other, or from one part of the connecting wire of the Voltaic apparatus to the other; for, let us suppose AB, Fig. 3, to represent a part of the above wire, and that the magnetic fluid passes in a straight line from A to B; and let CD represent one end of a dipping needle, in which the fluid passes from C to D—what can occasion the needle to *descend* in the direction from 1 to 2 when on one side of the wire, but to *ascend* in the direction from 2 to 1 when on the other side of the wire; and also to move from right to left when above the wire, but from left to right when below it; and further, that these effects are reversed, if we reverse either the poles of the wire or those of the needle?

But if it be granted that the fluid rotates, as I have suggested, then, to me at least, those movements and rotations are easily explained. I am, Gentlemen, yours, &c.

Dorset Street. J. Tatum.

Erratum.—In Mr. Tatum's last paper, vol. lxi. p. 245, line 24, after 'equator' *insert* 'of the'.

XXI. *True apparent Right Ascension of Dr.* Maskelyne's 36 *Stars for every Day in the Year* 1823, *at the Time of passing the Meridian of Greenwich.* [Continued from page 17.]

1823.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 53	H. M. 4 25	H. M. 5 3	H. M. 5 6	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 30	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 40	H. M. 11 41	H. M. 13 15	H. M. 14 7
Sept.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	12·21	17·08	5·89	49·80	41·69	4·72	10·02	38·21	22·62	20·06	4·13	30·95	54·85	58·07	3·36	30·49	54·93	37·54
2	23	10	92	83	73	75	05	23	65	09	16	98	86	09	36	49	92	52
3	24	13	94	86	77	77	08	26	67	12	18	31·01	88	10	36	49	92	51
4	25	15	97	89	81	80	12	29	70	15	21	03	89	11	36	49	91	50
5	27	18	6·00	92	86	83	15	32	73	18	23	06	91	12	36	49	90	49
6	28	21	02	95	90	86	19	35	75	21	25	09	92	14	37	50	90	47
7	29	23	05	98	94	89	22	38	78	24	28	12	94	15	37	50	89	46
8	30	26	08	50·01	98	92	25	41	81	27	31	15	96	16	37	50	88	45
9	32	28	10	04	42·02	95	29	44	83	30	33	18	97	18	37	50	88	44
10	33	30	13	07	07	98	32	47	86	33	36	20	99	19	38	51	87	43
11	34	33	15	10	11	5·01	36	50	89	36	38	23	55·01	21	38	51	87	42
12	35	35	18	13	15	03	39	53	92	39	41	26	03	22	38	51	86	41
13	36	37	20	16	19	06	42	56	95	42	43	29	04	24	39	52	86	40
14	37	39	23	19	23	09	46	59	97	45	46	32	06	25	39	52	85	39
15	38	42	25	22	28	12	49	62	23·00	49	49	35	08	27	40	53	85	38
16	39	44	28	25	32	15	53	65	03	52	51	38	10	28	40	53	84	37
17	40	46	30	28	36	18	56	68	06	55	54	41	11	30	* 41	54	84	36
18	41	48	32	31	40	21	59	71	09	58	57	44	13	32	42	* 55	84	35
19	42	50	35	34	44	24	63	74	12	61	60	47	15	33	43	56	84	34
20	43	52	37	37	48	26	66	77	15	65	62	50	17	35	43	56	83	33
21	43	54	39	40	52	29	69	80	18	68	65	53	19	37	44	57	83	33
22	44	56	41	43	56	32	72	83	20	71	68	56	21	39	45	58	83	32
23	45	58	44	45	61	35	76	86	23	74	71	59	23	40	46	59	83	31
24	45	60	46	48	65	38	79	89	26	78	74	62	25	42	47	60	83	30
25	46	62	48	51	69	40	82	92	29	81	76	66	27	44	48	61	82	29
26	47	63	50	54	73	43	86	95	32	84	79	69	30	46	49	62	82	29
27	47	65	52	57	77	46	89	98	35	88	82	72	32	48	50	63	82	28
28	48	67	54	60	81	49	92	39·01	38	91	85	75	34	50	51	64	82	27
29	48	69	56	63	85	52	96	04	41	95	88	78	37	52	52	65	82	27
30	48	70	58	66	89	54	99	07	44	98	90	82	39	54	53	66	82	26

Sept.	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 56	H. M. 23 59
	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	57·52	8·95	14·10	36·09	37·83	37·82	46·37	59·62	54·47	12·64	41·05	54·27	18·10	27·45	45·77	56·04	1·29	19·69
2	51	94	08	07	82	80	35	60	46	63	04	26	09	44	77	05	30	70
3	49	92	06	06	80	78	34	58	45	62	03	25	08	42	77	05	31	72
4	48	91	04	04	78	76	32	56	44	61	02	25	08	41	77	06	31	73
5	47	90	03	03	77	75	30	54	42	60	01	24	07	40	78	07	32	74
6	45	88	01	01	75	73	28	51	41	59	00	23	06	39	78	07	33	76
7	44	87	13·99	00	73	71	27	50	40	58	40·99	22	05	37	78	08	33	77
8	43	86	97	35·98	71	69	25	48	39	57	98	21	04	35	78	09	34	78
9	41	84	95	97	70	68	24	45	38	56	97	20	03	34	78	09	34	80
10	40	83	94	95	68	66	22	43	36	55	96	19	02	32	78	10	35	81
11	39	82	92	94	66	64	20	41	35	54	94	18	01	31	78	10	35	82
12	38	81	90	92	64	63	19	38	34	52	93	17	00	29	77	10	35	83
13	37	80	88	91	63	61	17	36	33	51	92	16	17·99	27	77	10	36	84
14	35	78	86	89	61	59	15	33	31	50	91	15	98	26	77	11	36	85
15	34	77	85	88	59	57	13	31	30	49	90	13	96	24	77	11	36	86
16	33	76	83	86	58	56	12	29	29	47	88	12	95	22	76	11	37	87
17	32	75	81	85	56	54	10	26	27	46	87	11	94	20	76	11	37	88
18	31	74	79	83	54	52	08	24	26	45	86	10	93	18	76	11	37	89
19	30	73	78	82	53	50	06	21	24	43	84	09	92	16	75	10	37	90
20	29	72	76	81	51	49	05	19	23	42	83	07	90	14	75	10	38	91
21	28	71	75	79	49	47	03	16	21	40	82	06	89	12	74	10	38	91
22	27	70	73	78	48	45	01	14	20	39	80	05	88	10	74	10	38	92
23	26	69	72	77	46	43	45·99	11	18	37	79	04	87	08	73	10	38	93
24	25	68	70	76	45	41	97	09	17	36	77	02	85	06	73	09	37	93
25	25	68	69	74	43	40	96	06	15	34	76	01	84	04	72	09	37	94
26	24	67	67	73	42	38	94	04	14	33	75	00	83	02	72	09	37	95
27	23	66	66	72	40	36	92	01	12	31	73	53·98	81	00	71	09	37	95
28	22	65	64	71	38	34	90	58·98	10	29	71	97	80	26·98	70	09	37	96
29	22	65	63	70	37	32	88	96	09	28	70	95	78	96	70	08	36	96
30	21	64	62	68	36	31	87	93	07	26	68	94	77	93	69	08	36	96

N. B. On those days where an Asterisk is prefixed the Star passes twice; the Æ given is that at the first passage.

XXII. *An Account of the remarkable Accumulation of the Exuviæ of Bears, in a Cave at Kühloch in Franconia. By Professor* Buckland*.

THE cave of Kühloch (in Franconia), is more remarkable than all the rest, as being the only one I have ever seen, excepting that of Kirkdale, in which the animal remains have escaped disturbance by diluvial action; and the only one also in which I could find the black animal earth, said by other writers to occur so generally, and for which many of them appear to have mistaken the diluvial sediment in which the bones are so universally imbedded. The only thing at all like it, that I could find in any of the other caverns, were fragments of highly decayed bone, which occurred in the loose part of the diluvial sediment in the caves of Scharzfeld and Gailenreuth; but in the cave of Kühloch it is far otherwise. It is literally true that in this single cavern (the size and proportions of which are nearly equal to those of the interior of a large church) there are hundreds of cart loads of black animal dust entirely covering the whole floor, to a depth which must average at least six feet, and which, if we multiply this depth by the length and breadth of the cavern, will be found to exceed 5000 cubic feet. The whole of this mass has been again and again dug over in search of teeth and bones, which it still contains abundantly, though in broken fragments. The state of these is very different from that of the bones we find in any of the other caverns, being of a black, or, more properly speaking, dark umber colour throughout, and many of them readily crumbling under the finger into a soft dark powder, resembling mummy powder, and being of the same nature with the black earth in which they are imbedded. The quantity of animal matter accumulated on this floor is the most surprising, and the only thing of the kind I ever witnessed; and many hundred, I may say thousand, individuals must have contributed their remains to make up this appalling mass of the dust of death. It seems in great part to be derived from comminuted and pulverised bone; for the fleshy parts of animal bodies produce by their decomposition so small a quantity of permanent earthy residuum, that we must seek for the origin of this mass principally in decayed bones. The cave is so dry, that the black earth lies in the state of loose powder, and rises in dust under the

* This article is extracted from the section on the Caves in Franconia in Professor Buckland's "*Reliquiæ Diluvianæ*," lately published; of which interesting work, and several recent memoirs on subjects connected with those which are discussed in it, we purpose to give analyses in our next.

feet:

feet: it also retains so large a proportion of its original animal matter, that it is occasionally used by the peasants as an enriching manure for the adjacent meadows*.

The exterior of this cavern presents a lofty arch, in a nearly perpendicular cliff, which forms the left flank of the gorge of the Esbach, opposite the Castle of Rabenstein. The depth of the valley below it is less than 30 feet, whilst above it the hill rises rapidly, and sometimes precipitously, to 150 or 200 feet. This narrow valley or gorge is simply a valley of denudation, by which the waters of the Esbach fall into those of the Weissent. The breadth of the entrance arch is about 30 feet, its height 20 feet. As we advance inwards the cave increases in height and breadth, and near its inner extremity divides into two large and lofty chambers, both of which terminate in a close round end, or *cul de sac*, at the distance of about 100 feet from the entrance. It is intersected by no fissures, and has no lateral communications connecting it with any other caverns, except one small hole close to its mouth, and which opens also to the valley. These circumstances are important, as they will assist to explain the peculiarly undisturbed state in which the interior of this cavern has remained, amid the diluvial changes that have affected so many others. The inclination of the floor, for about 30 feet nearest the mouth, is very considerable, and but little earth is lodged upon it; but further in, the interior of the cavern is entirely covered with a mass of dark brown or blackish earth, through which are disseminated in great abundance the bones and teeth of bears and other animals, and a few small fragments of limestone, which have probably fallen from the roof; but I could find no rolled pebbles. The upper portion of this earth seems to be mixed up with a quantity of calcareous loam, which, before it had been disturbed, by digging, probably formed a bed of diluvial sediment over the animal remains; but as we sink deeper, the earth gets blacker, and more free from loam, and seems wholly composed of decayed animal matter. There is no appearance of either stalactite or stalagmite having ever existed within this cavern.

In some of the particulars here enumerated, there is an apparent inconsistency with the phænomena of other caverns; but the differences are such as arise from the particular position

* I have stated, that the total quantity of animal matter that lies within this cavern cannot be computed at less than 5000 cubic feet. Now allowing two cubic feet of dust and bones for each individual animal, we shall have in this single vault the remains of at least 2500 bears, a number which may have been supplied in the space of 1000 years, by a mortality at the rate of two and a half per annum.

and circumstances of the cave at Kühloch: the absence of pebbles, and the presence of such an enormous mass of animal dust, are the anomalies I allude to; and both these circumstances indicate a less powerful action of diluvial waters within this cave than in any other, excepting Kirkdale. To these waters, however, we must still refer the introduction of the brown loam, and the formation or laying open of the present mouth of the cavern; from its low position so near the bottom of the valley, this mouth could not have been exposed in its present state, and indeed must have been entirely covered under the solid rock, till all the materials that lay above it had been swept away, and the valley cut down nearly to its present base; and as the cave ends inwardly in a *cul de sac*, and there is no vertical fissure, or any other mode of access to it, but by the present mouth, if we can find therein any circumstances that would prevent the admission of pebbles from without, or the removal of the animal remains from within, the cause of the anomaly we are considering will be explained. The throat of the cave, by which we ascend from the mouth to the interior, is highly inclined upwards, so that neither would any pebbles that were drifting on with the waters that excavated the valley, ascend this inclined plane to enter the cave, nor would the external currents, however rapidly rushing by the outside of the mouth, have power to agitate (except by slight eddies in the lower part of the throat) the still waters that would fill the bottom of the cavern, and which being there quiescent, would, as at Kirkdale, deposit a sediment from the mud suspended in them upon the undisturbed remains of whatever kind that lay on the floor. From its low position, it is also probable that this vault formed the deepest recess of an extensive range of inhabited caves, to which successive generations of antediluvian bears withdrew themselves from the turbulent company of their fellows, as they felt sickness and death approaching; the habit of domesticated beasts and birds to retire and hide themselves on the approach of death, renders it probable that wild and savage animals also do the same. The unusual state of decay of the teeth and bones in this black earth may be attributed to the exposed state of this cavern arising from its large mouth and proximity to the external atmosphere, and to the absence of that protection which in closer and deeper caves they have received, by being secluded from such exposure, or imbedded in more argillaceous earth, or invested with and entirely sealed up beneath a crust of stalagmite.

XXIII. *Ob-*

XXIII. *Observations upon the Cadmia found at the Ancram Iron-Works in Columbia County, New-York, erroneously supposed to be a new Mineral.* *By* WM. H. KEATING*.

IN the second number of the first volume of the New-York Medical and Physical Journal, Dr. Torrey has published a description and analysis of a substance, which he considered as a new mineral, and for which he proposed the name of green oxide of zinc: a specimen of this substance having been handed to me last spring, I immediately recognised it to be similar in its nature and appearance to a product of the iron furnaces of Belgium, which has been described by Mr. Bouesnel in the "*Journal des Mines*," (vol. xxix. p. 35) under the name of Cadmia. Having had an opportunity of collecting on the spot† the most satisfactory proofs in support of my opinion, I beg leave to offer to the Academy the following account of this substance. It was first noticed at Ancram in the year 1812, when it was found in pulling down a stone wall connected with the iron furnace, which belongs to General Livingston, and is now under the direction of Walter Patterson, Esq. It excited some interest among the mineralogists of New-York, but no public notice was taken of it until lately. Mr. Bouesnel's observations on this subject are very full; these and a few short notes by Messrs. Collet Descotils, Heron de Villefosse and Berthier in the *Journal* and in the *Annales des Mines*, are the only notices of it I have ever met with; I sought in vain for a mention of it in English works. The cadmia of Belgium is a new and rare metallurgical product, which is formed in iron furnaces about five or six feet below their orifice, and immediately under the charge; it there forms an annular disk or ring, which increases continually in thickness, and which, if not removed, would choke the furnace; it forms in the Belgian furnaces, according to Mr. Bouesnel, a ring of about sixteen inches in height, offering in the profile or vertical section, a curvilineal triangle, the base of which rests upon the sides of the furnace; and the apex which corresponds with its greatest breadth, is but little distant from the lower part of the ring, so that the triangle appears in some cases almost rectangular." I have seen a piece found at Ancram, which presented tolerably well the above described characters, and corresponded exactly with Mr. Bouesnel's description; like the European, it

* Silliman's American Journal of Science, &c. vol. vi. p. 180, from the Journal of the Academy of Natural Sciences of Philadelphia, vol. ii. Part II.

† These observations were made during a short visit to Ancram, in company with Mr. Vanuxem, who likewise, at the first inspection, recognised this substance to be cadmia.

it was found in tabular masses, presenting in many cases a distinct slaty structure. The substance has often a striped aspect; its colour is grayish, inclining to yellow, green or black. The specific gravity of the European is 5·25, of the American 4·92; this difference is not very great, and may in part be accounted for, by the fact that the former contains a small quantity of lead, which varies from 2·4 to 6·0 per 100·0.

The chemical analysis of this substance made in New-York, has rendered it unnecessary for me to undertake that which I proposed making. I shall merely add a comparative view of the results of the analyses, made upon the European and American.

	Bouesnel.	Drappier.	Berthier.	Torrey.
Oxide of zinc	90·1	94·0	87·0	93·5
——— lead	6·0	2·4	4·9	
——— iron	1·6	2·6	3·6	3·5
Carbon	1·0	0·5	0·6	1·0
Silex, earths, sand, &c.	1·8		3·4	
	100·5	99·5	99·5	98·0

These analyses present a remarkable coincidence, except in the presence of lead in the European, and its absence in the American cadmia; but this difference is of no importance; in Belgium Mr. Bouesnel tells us that the iron ore is visibly intermixed with lead ore, and this accounts for its existence in the cadmia; we are also told that lead is found there in the furnaces below the metallic iron. It is not difficult to account for the presence of zinc with the iron ore; for in examining the ore bed at Salisbury (14 miles east of the furnace) we ascertained that the hematite was found in the side of a hill, incumbent upon the schist, and, as it were, incased in the decomposed part of it, and that the adjoining schist was very much broken up and altered: it does not appear that the hematite is the result of infiltration alone, for masses of micaceous iron ore are found connected with it, which appear to indicate that it results, in part at least, from the decomposition of oxidule or oligist iron ore. We know that this schist contains blende or sulphuret of zinc, in some places at least, as at the Ancram lead-works, and this may account for the presence of zinc.

Mr. Bouesnel has endeavoured to explain the formation of these cadmia, in a manner which does not appear to me to be satisfactory. I would rather admit that it results from a reduction of the oxide or carbonate of zinc, which is intermixed in small quantities with the iron ore; that this reduction takes place in the furnace; that the zinc sublimes and oxidates as it rises, and settles in the form of a ring at the inferior part of the

the charge, where the temperature of the furnace is considerably lowered by the successive additions of cold ore, charcoal, &c.

This substance is not, it is true, found at present forming in the Ancram furnace; but this may in a great measure be owing to a better roasting of the ore, previous to its introduction into the furnace. It may also be occasioned by the circumstance that all the ore destined for Ancram is picked with great care at the ore bed. I must not, however, omit to state, that I found in the flue erected above the orifice of the furnace, for the protection of the workmen, a red, pulverulent substance, to which the workmen have given the name of *sulphur*, a name which, as the editor of the Emporium has well observed, has been most unfortunately given by furnace and forge men, to every product which puzzles them, and without any regard to its real composition: this powder I supposed to be a mixture of ashes and fine ore, blown out of the furnace by the rapid current of air; I conceived that if there was any zinc with the ore, it would be likely to be detected in this substance; accordingly I found by analysis, about eight per cent. of oxide of zinc, a quantity much greater than I expected. It would require a more accurate study of the progress of the furnace than I could make in two days, and a better knowledge of the methods formerly in use, to determine why cadmia are not formed there at present, as they were formerly. Dr. Torrey has, I believe, never visited Ancram, and the information which he received on the subject may have led him into error. For instance, he was misinformed (I think) when he stated, that "it was found when taking down one of the old walls of the furnace erected in the year 1744." We were told by Mr. Patterson, that it had never been found but in taking down a wall *connected* with the furnace, and which having been built after the furnace, may have contained materials which had been extracted from it at different times. This observation is of more importance than it at first appears; for if, as Mr. Patterson told us, the Ancram furnace was the first erected in the colonies of North America, or at least the first in the province of New-York, and if, according to Dr. Torrey, the cadmia had been found in the wall of the first furnace erected, the substance must have pre-existed to any furnace known to have been erected there, which we think is not the case.

But, in addition to all the above-mentioned proofs, and to those which might be drawn from the circumstance of its being found in the vicinity of a furnace, I have been able to obtain the evidence of men to the fact of its having been formed in it.

Having

Having been informed that ore from the same bed was used at the works belonging to Messrs. Holley and Coffing, near Salisbury, I repaired there with a hope of finding the cadmia near that furnace also. After a short search, I found it in its immediate vicinity, and was informed by Mr. Holley, that he had himself taken it out of his furnace about twelve years ago, when they renewed the stack. He was positive that it was the same; that it had been found about six feet below the orifice of the furnace, and that if not occasionally removed, it would have eventually choked it. I even understood him or his partner to say, that this substance was even at present occasionally formed in the furnace in pieces of almost one-eighth of an inch in thickness. One of the reasons why it is still formed at Salisbury, and not at Ancram, is probably owing to the ore used at Ancram being picked, and the other not. Mr. Patterson thinks his ore is also better roasted.

According to Mr. Heron de Villefosse, a similar substance is formed in the copper and lead furnaces of Julius, Sophia, and Ocker, near Goslar, in the Hartz. At Goslar, as well as at Jemmapes in Belgium, this cadmia is considered as the best material that could be used in the manufacture of brass; as it is purer than the roasted calamine, it is preferred to it, as well as to all other zinciferous substances. It had not, I believe, been used in Belgium before Mr. Bouesnel described it. Should it be found in any quantity at our furnaces, it would no doubt be equally advantageous to work it with copper for brass.

This substance has not yet been observed in many places. I believe the only spot where it has been noticed, in addition to the above mentioned, is at Verrieres, in France, where I discovered it in the year 1819*. I am inclined to think that if more care were taken by our iron-masters in observing the progress of their furnaces, and the products which they yield,

* As no account of the cadmia of Verrieres has as yet been published, I shall here add the note which I made on the subject in my journal. "July 6, 1819. I visited the furnace of Verrieres, in the department de la Vienne, in France. The director mentioned that his ore was good, and that the iron it produced was likewise good. He complained, however, of a substance which formed in the furnace, five feet below its orifice; it was in the form of a ring. It would, he said, have choked the furnace if not removed, which at times was a difficult undertaking. I mentioned to him that it appeared to be analogous to the cadmia of Belgium. The specimens which I took with me were heavy, compact, and of a dark colour." —I have not had an opportunity of analysing them since; but my suspicions on this subject were confirmed, when, on returning to Paris in the autumn of 1820, I was informed that the engineer of mines De Cressac had discovered calamine in that vicinity the year before.

it might be found in many other places; certainly it must have been formed in the old Franklin furnace, in Sussex county New-Jersey, where so many fruitless attempts were made to work the Franklinite.

Before I conclude these remarks, I must observe, that it does not appear that the presence of zinc affects the properties of iron. In Belgium the iron is of good quality; and it is an interesting fact, that the bar-iron of Ancram is in great demand at $ 120 per ton, a higher price than is at present paid for any imported iron. The castings from the Ancram furnace are in such a repute, that no other pigs are used at the West Point Foundry for the heavy guns (32 and 42 pounders) now casting for the United States' navy.

The Ancram furnace equals, in beauty of workmanship, and economy of means, any that we have seen; and we entertain no doubt, that all works carried on with such admirable perfection, must and will always prove equally honourable and profitable to their owners and directors.

XXIV. *On a Planetary Analogy; or a Law of Motion pervading and connecting all the Planetary Orbits.* By *Mr.* J. Utting.

To the Editors of the Philosophical Magazine and Journal.

Lynn Regis, June 21, 1823.

THE following beautiful analogy which obtains in the motions of the planetary orbs has, I believe, never been described by any astronomical writer, or is not generally known, viz. If the mean orbicular motion of each planet in its orbit, be multiplied by the square root of its mean distance from the sun, a product will be obtained common to all the planets: for instance, if the orbicular motion in miles of each planet in one sidereal day, be multiplied by the square root of its mean distance from the sun, the product will be 15.634.588.170 miles, a *constant* quantity for all the planets, as the mean velocity of the planets, multiplied by the square root of their respective mean distance, is always a *constant* quantity.

The same analogy obtains in each respective system of satellites; for, if the velocity of a satellite be multiplied by the square root of its mean distance from its primary, a *constant* product will also be produced in each respective system of satellites; and if this *constant* product be multiplied by the square root of the reciprocal of the sun's attractive power, and that of their respective primaries, the same result will be produced as that which obtains in the planetary motions, as above. Thus a *constant* product, or *quantity*, obtains in the motions of

of the planets, and their respective systems of satellites, extending to the whole planetary system, resulting from the periodic times and mean distances of the planets, with the periodic times and mean distances of their satellites, compounded with the attractive power of the sun, as compared with that of the primary planets, around which each respective system of satellites circulates; viz.

Let V, V′, V″, &c. represent the velocities of the planets in their orbits; and $\sqrt{D}$, $\sqrt{D'}$, $\sqrt{D''}$, &c. the square roots of their mean distances from the sun. Let also v, v', v'', &c. represent the velocities of their respective satellites; and the $\sqrt{d}$, $\sqrt{d'}$, $\sqrt{d''}$, &c. the square roots of their mean distances from their primaries. Let the square root of the sun's attractive power, that of each planet being unity, be denoted by $\sqrt{m}$, $\sqrt{m'}$, $\sqrt{m''}$, &c. respectively.

Whence we have $V \times \sqrt{D} = V' \times \sqrt{D'} = V'' \times \sqrt{D''}$ &c. a *constant* quantity for the primary planets. And $v \times \sqrt{d} = v' \times \sqrt{d'} = v'' \times \sqrt{d''}$, &c. a constant quantity for each respective system of satellites. Also, $v \times \sqrt{d} \times \sqrt{m}. = v' \times \sqrt{d'} \times \sqrt{m'}. = v'' \times \sqrt{d''} \times \sqrt{m''}$, &c. a *constant* quantity equal to that in the first analogy. Whence $\dfrac{V \times \sqrt{D}}{v \times \sqrt{d} \times \sqrt{m}} = 0. = \dfrac{V' \times \sqrt{D'}}{v' \times \sqrt{d'} \times \sqrt{m'}} = 0. = \dfrac{V'' \times \sqrt{D''}}{v'' \times \sqrt{d''} \times \sqrt{m''}} = 0.$ &c.

The following general analogy also obtains, viz. As $V : V' :: \sqrt{D'} : \sqrt{D}$; also as $v : v' :: \sqrt{d'} : \sqrt{d}$ &c. where the product of the two extreme terms will always be equal to the product of the two mean ones, whatever may be the planets or satellites fixed on.

The following table exhibits the result of my calculations in elucidation of this analogy.

Tabular View of the Analogy which obtains in the Planetary System.

Planets.	Sidereal periods in sid. days.	Mean dist. in miles.	Mass of the ☉ each planet = 1.	Square root of the ☉'s mass.	Square root of mean dist. in miles.	Velocity in one sidereal day in miles.	Constant product in miles.
☿	88·2101005	36·387308	......	......	6032·19	× 2591860	= 15634588170
♀	225·3159734	67·993235	......	......	8245·80	× 1896067	= 15634588170
⊕	366·2563835	94·000000	337102·	580·605	9695·36	× 1612585	= 15634588170
♂	688·8604607	143·227108	......	......	11967·75	× 1306393	= 15634588170
♃	4344·4468810	488·908265	1066·09	32·6510	22111·27	× 707087	= 15634588170
♄	10787·7763273	896·517987	3512·08	59·2628	29941·91	× 522164	= 15634588170
♅	30772·7323350	1803·218792	19504·	139·6567	42464·32	× 368182	= 15634588170

Satel-

Satellite of the Earth.

Satellites.	Sid. period in sid. days.	Mean dist. in miles.	Square root of mean dist. in miles.	Velocity in one sid. day in miles.	Constant product for each system of satellites in miles.	Square root of the ⊙'s mass.	Constant product in miles.
☽	27·3964621	239780	489·674 ×	54992 =	26928100 ×	580·605 =	15634588170
			Satellites of Jupiter.				
1	1·7739813	263410	513·239 ×	932975 =	478839200 ×	32·6510 =	15634588170
2	3·5609034	419160	647·426 ×	739605 =	478839200 ×	32·6510 =	15634588170
3	7·1741405	668630	817·699 ×	585594 =	478839200 ×	32·6510 =	15634588170
4	16·7344602	1176020	1084·444 ×	441553 =	478839200 ×	32·6510 =	15634588170
			Satellites, and Ring of Saturn.				
1	0·9452910	116360	341·112 ×	773406 =	263818000 ×	59·2628 =	15634588170
2	1·3739915	149300	386·400 ×	682760 =	263818000 ×	59·2628 =	15634588170
3	1·8929684	184860	429·965 ×	613595 =	263818000 ×	59·2628 =	15634588170
4	2·7469802	236950	486·775 ×	541973 =	263818000 ×	59·2628 =	15634588170
5	4·5298580	330730	575·091 ×	458741 =	263818000 ×	59·2628 =	15634588170
6	15·9889550	766710	875·618 ×	301293 =	263818000 ×	59·2628 =	15634588170
7	79·5467885	2234420	1494·798 ×	176491 =	263818000 ×	59·2628 =	15634588170
Ring of ♄	0·4402692	69914	264·412 ×	997754 =	263818000 ×	59·2628 =	15634588170
			Satellites of Uranus.				
1	5·9087328	222960	472·186 ×	237089 =	111950150 ×	139·6567 =	15634588170
2	8·7306375	289240	537·812 ×	208159 =	111950150 ×	139·6567 =	15634588170
3	10·9911093	337230	580·714 ×	192780 =	111950150 ×	139·6567 =	15634588170
4	13·4927396	386630	621·797 ×	180043 =	111950150 ×	139·6567 =	15634588170
5	38·1792417	773480	879·476 ×	127292 =	111950150 ×	139·6567 =	15634588170
6	107·9892458	1546980	1243·775 ×	90008 =	111950150 ×	139·6567 =	15634588170

Note.—The periodic times of the planets and satellites were taken from the fourth edition of Laplace's *Système du Monde*, the time being converted from solar to sidereal days in the proportion of 1·0027378 to 1. The mass or *attractive* power of the sun, and planets, was also taken from the same work, from which with the periodic time, and *constant* product, the *distances* of all the satellites from their primaries were *computed*. The distance of the ring of Saturn, is the distance from the centre of the planet to the centre of attraction in the cylinder of the ring, or the centre of gravity of a satellite, supposing all the particles of matter in the ring to be condensed into a globular form, and whose sidereal period is equal to that of the rotation of the ring.

XXV. *State of the Thermometer at Smyrna for every Day in the Year* 1820 (*being the Year of the great Eclipse, and Leap-year,*) *taken in the Shade four times every Day; viz.* 9 *A.M., Noon,* 6 *P.M., Midnight. Communicated from Smyrna by a Correspondent to Dr.* T. Forster.

STATE OF THE THERMOMETER AT SMYRNA. 1820.

	JANUARY.				FEBRUARY.				MARCH.				APRIL.				MAY.				JUNE.			
	9Morn.	Noon.	6Even.	Night.	9Morn.	Noon.	6Even.	Night.	9Morn.	Noon.	6Even.	Night.	9Morn.	Noon.	6 Even.	Night.	9Morn.	Noon.	6 Even.	Night.	9Morn.	Noon.	6 Even.	Night.
1	50	53	55	52	43	48	49	46	34	44	48	50	45	50	55	50	67	72	75	68	63	70	72	62
2	51	54	55	50	44	50	52	48	36	38	37	38	50	60	62	55	66	70	69	65	63	73	71	66
3	50	55	56	51	47	57	58	52	37	39	42	38	55	65	67	50	59	60	63	55	64	75	74	65
4	52	56	55	50	55	58	56	54	36	39	42	39	50	52	50	49	55	60	64	57	65	74	77	64
5	53	55	54	57	55	56	54	50	37	40	42	40	47	50	52	47	57	61	63	58	67	80	82	65
6	40	45	48	42	50	52	50	49	39	44	43	42	45	55	60	52	58	62	64	59	68	80	83	70
7	42	48	50	42	48	50	52	49	49	52	53	52	50	65	67	65	57	64	65	60	68	78	82	70
8	40	49	47	43	47	52	54	50	50	53	50	48	57	67	69	67	59	65	66	62	70	82	80	75
9	42	50	51	42	49	54	55	51	45	50	49	48	58	68	69	65	60	66	68	63	70	82	81	77
10	39	48	49	40	52	60	62	55	44	52	51	50	62	67	65	60	62	70	72	64	72	83	80	76
11	37	47	45	38	58	62	65	60	49	54	53	50	52	55	52	47	63	72	70	60	72	84	85	76
12	36	46	49	40	60	62	65	55	45	50	51	47	47	52	55	50	60	65	66	60	73	85	87	73
13	40	50	52	50	55	60	65	60	47	52	53	50	49	53	52	48	50	55	54	50	74	86	87	70
14	45	50	55	50	56	60	63	60	48	52	53	50	50	60	62	60	50	60	62	52	75	85	82	74
15	45	52	50	48	56	60	62	60	48	53	54	48	50	60	62	58	56	66	67	56	72	84	82	75
16	46	50	51	48	57	62	63	58	45	50	49	47	55	60	63	58	55	65	68	58	74	83	80	72
17	48	55	56	52	58	65	64	59	44	47	48	45	57	62	64	58	60	70	71	60	74	80	82	70
18	55	58	57	53	57	64	65	58	44	47	48	46	60	64	65	60	60	71	72	60	72	82	81	70
19	55	57	56	50	52	51	50	48	40	44	45	41	60	65	64	61	61	75	78	65	72	80	82	69
20	49	57	56	50	47	48	47	49	42	45	44	42	62	66	65	62	65	76	79	66	73	85	86	70
21	48	55	50	49	48	47	49	48	40	49	50	49	62	70	72	68	66	71	79	69	75	85	87	72
22	45	52	50	48	48	50	51	50	49	55	56	50	67	72	73	70	78	80	82	70	77	87	88	72
23	45	50	52	49	49	52	53	50	55	57	56	52	69	73	74	70	72	80	84	74	77	87	88	73
24	47	57	50	49	49	55	53	50	53	52	50	49	70	75	76	70	74	84	85	75	78	88	87	74
25	45	55	50	49	49	56	54	50	49	50	51	48	71	76	75	71	75	85	86	76	78	88	87	73
26	46	56	51	48	50	53	50	49	48	49	50	49	70	72	73	70	74	85	84	75	78	88	88	74
27	48	58	56	50	47	45	43	42	45	49	50	47	68	72	71	68	73	86	85	80	79	88	89	87
28	50	59	58	52	42	45	46	39	42	50	52	48	64	74	72	69	74	84	86	80	79	89	90	76
29	52	60	57	50	35	47	48	40	50	52	53	50	62	72	74	70	76	85	86	79	79	88	89	77
30	53	60	62	50					52	54	55	50	67	70	75	69	76	87	88	80	78	88	87	76
31	48	47	45	45					50	52	50	46					76	80	75	72				

	9Morn.	Noon.	6 Even.	Night.	9Morn.	Noon.	6 Even.	Night.	9Morn.	Noon.	6Even.	Night.	9Morn.	Noon.	6Even.	Night.	9Morn.	Noon.	6 Even.	Night.	9Morn.	Noon.	6Even.	Night.
1	77	88	87	75	75	85	84	80	77	87	89	75	76	86	85	75	55	59	61	55	60	65	68	66
2	78	89	87	76	74	86	85	78	76	86	87	75	75	85	86	76	56	60	62	58	60	66	70	60
3	77	88	87	76	74	87	85	84	76	86	88	75	76	87	88	76	58	62	63	59	50	52	55	50
4	77	89	86	77	78	86	88	77	76	86	88	76	76	87	88	77	57	60	62	56	55	60	62	60
5	78	90	91	78	78	87	86	80	76	86	88	76	75	85	87	76	54	56	57	56	56	62	65	50
6	77	87	90	75	74	84	80	76	76	87	89	76	76	86	87	76	55	57	58	57	45	50	52	48
7	75	85	87	80	75	84	69	76	77	87	89	76	75	85	86	75	57	58	59	57	48	52	50	48
8	74	84	85	77	75	85	86	76	77	87	89	76	76	86	87	74	55	60	62	56	48	55	56	50
9	75	85	80	75	80	87	88	78	77	88	89	75	75	85	86	75	52	60	63	57	50	60	59	58
10	75	88	87	74	82	88	89	80	76	87	88	76	70	80	85	73	53	62	64	65	52	62	63	60
11	76	86	85	77	80	89	90	81	76	86	88	76	72	82	85	72	52	60	63	56	50	60	62	55
12	77	90	92	76	78	88	89	80	75	89	86	76	73	83	84	70	55	63	64	57	50	60	63	53
13	75	85	87	77	78	88	89	80	76	86	89	75	74	84	85	70	56	64	65	57	50	55	56	50
14	76	86	88	78	78	89	90	80	76	87	89	76	72	82	84	69	57	65	66	58	52	60	62	55
15	76	86	88	80	75	85	80	77	76	86	88	75	70	80	82	68	58	65	67	57	50	49	48	45
16	76	87	88	75	77	87	88	75	76	86	88	75	72	82	83	70	57	67	66	56	45	50	51	45
17	75	85	86	76	78	88	89	76	75	85	87	76	73	83	84	70	57	67	68	57	50	53	55	52
18	76	86	85	75	78	88	89	77	74	84	85	74	74	84	86	72	58	65	67	56	52	54	56	52
19	77	88	90	78	76	86	88	78	70	82	84	70	73	83	86	74	57	67	68	58	51	55	57	53
20	78	88	90	79	77	88	89	76	70	80	82	67	74	84	80	70	58	67	68	56	52	56	58	55
21	78	89	89	78	78	88	89	78	66	76	72	68	70	72	70	68	57	66	67	57	50	55	53	50
22	77	88	89	78	80	89	85	75	64	74	75	66	68	70	72	67	56	66	67	58	55	60	62	55
23	78	89	90	78	75	85	86	76	63	73	74	64	65	69	68	66	57	67	68	57	56	66	67	60
24	77	88	89	77	76	86	87	75	64	74	75	65	65	68	60	64	55	60	62	56	50	56	57	58
25	78	89	90	78	76	86	87	75	65	75	77	67	64	66	68	62	56	62	63	55	52	57	56	55
26	79	89	90	78	75	86	87	75	68	78	77	60	62	65	67	60	57	63	64	56	48	50	55	48
27	80	90	91	78	75	87	88	76	70	78	80	72	62	64	65	60	55	62	63	57	45	48	50	49
28	80	90	91	78	76	88	87	75	75	85	86	75	60	64	63	59	56	63	64	55	47	49	50	50
29	82	92	93	80	74	85	86	74	78	88	89	72	59	63	64	59	56	65	70	58	48	50	52	50
30	83	93	95	82	75	85	87	75	77	87	88	76	58	62	63	58	60	70	69	59	50	55	57	52
31	79	85	83	78	76	87	86	75					55	61	62	57					50	55	56	50

XXVI. *No-*

XXVI. *Notice of the Fusion of Plumbago, or Graphite, (commonly called Black Lead,) in a Letter from Professor* Silliman *to Professor* Robert Hare, *M.D. Dated March* 26, 1823.*

In a former letter published in my Journal, (vol. v. p. 108,) and in an additional notice, (p. 361 same vol.) I gave an account of the fusion and volatilization of charcoal, by the use of your Galvanic Deflagrator. I have now to add, that the fusion of plumbago was accomplished yesterday by the same instrument, and that I have again obtained the same results to-day. For this purpose, from a piece of very fine and beautiful plumbago, from North Carolina, I sawed small parallelopipeds, about one eighth of an inch in diameter, and from three fourths of an inch to one inch and a quarter in length: these were sharpened at one end, and one of them was employed to point one pole of the deflagrator, while the other was terminated by prepared charcoal. Plumbago being in its natural state a conductor, (although inferior to prepared charcoal,) a spark was readily obtained, but in no instance of half the energy which belongs to the instrument when in full activity; for the zinc coils were very much corroded, and some of them had failed and dropped out; still the influence was readily conveyed through the remaining coils. As my hopes of success, in the actual state of the instrument, were not very sanguine, I was the more gratified to find a decided result in the very first trial. To avoid repetitions, I will generalise the results. The best were obtained when the plumbago was connected with the copper, and prepared charcoal with the zinc pole. The spark was vivid, and globules of melted plumbago could be discerned, even in the midst of the ignition, *forming* and *formed* upon the edges of the focus of heat. In this region also there was a bright scintillation, evidently owing to combustion, which went on where air had free access, but was prevented by the vapour of carbon, which occupied the highly luminous region of the focus, between the poles, and of the direct route between them. Just on and beyond the confines of the ignited portion of the plumbago, there was formed a belt of a reddish brown colour, a quarter of an inch or more in diameter, which appeared to be owing to the iron remaining from the combustion of the carbon of that part of the piece, and which, being now oxidized to a maximum, assumed the usual colour of the peroxide of that metal.

In various trials, the globules were formed very abundantly

* Silliman's Journal, vol. vi. p. 341.

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on the edge of the focus, and in several instances were studded around so thickly, as to resemble a string of beads, of which the largest were of the size of the smallest shot; others were merely visible to the naked eye, and others still were microscopic. No globule ever appeared on the point of the plumbago, which had been in the focus of heat; but this point presented a hemispherical excavation, and the plumbago there had the appearance of black scoriæ or volcanic cinders. These were the general appearances at the copper pole occupied by the plumbago.

On the zinc pole, occupied by the prepared charcoal, there were very peculiar results. This pole was, in every instance, elongated towards the copper pole, and the black matter, accumulated there, presented every appearance of fusion, not into globules, but into a fibrous and striated form, like the half flowing slag found on the upper currents of lava. It was evidently transferred, in the state of vapour, from the plumbago of the other pole, and had been formed by the carbon taken from the hemispherical cavity. It was so different from the melted charcoal, described in my former communications, that its origin from the plumbago could admit of no reasonable doubt. I am now to state other appearances which have excited in my mind a very deep interest. On the end of the prepared charcoal, and occupying, frequently, an area of a quarter of an inch or more in diameter, were found numerous globules of perfectly melted matter, entirely spherical in their form, having a high vitreous lustre, and a great degree of beauty. Some of them, and generally they were those most remote from the focus, were of a jet black, like the most perfect obsidian; others were brown, yellow, and topaz coloured: others still were greyish white, like pearl stones with the translucence and lustre of porcelain; and others still, limpid like flint glass, or in some cases like hyalite or precious opal, but without the iridescence of the latter. Few of the globules upon the zinc pole were perfectly black, while very few of those on the copper pole were otherwise. In one instance, when I used some of the very pure English plumbago, (sawed from a cabinet specimen, and believed to be from Borrowdale,) white and transparent globules were formed on the copper side.

When the points were held *vertically, and the plumbago uppermost*, no globules were formed on the latter; and they were unusually numerous, and almost all black, on the opposite pole. When the points were exchanged, plumbago being on the zinc, and charcoal on the copper end, very few globules were formed on the plumbago, and not one on the charcoal: this last was rapidly hollowed out into a hemispherical cavity, while the plumbago

plumbago was as rapidly elongated by matter accumulating at its point, and which, when examined by the microscope, proved to be a concretion in the shape of a cauliflower—of volatilized and melted charcoal, having in a high degree all the characteristics which I formerly described as belonging to this substance. Indeed, I found by repetitions of the experiment, that this was the best mode of obtaining fine pieces of melted charcoal.

In some instances I used points of plumbago on both poles, and always obtained melted globules on both; the results were, however, not so distinct as when plumbago was on the copper and charcoal on the zinc pole; but the same elongation of the zinc and hollowing of the copper pole took place as before. I detached some of the globules, and partly bedding them in a handle of wood, tried their hardness and firmness; they bore strong pressure without breaking, and easily scratched, not only flint glass, but window glass, and even the hard green variety which forms the aqua fortis bottles. The globules which had acquired this extraordinary hardness, were formed from plumbago which was so soft that it was perfectly free from resistance when crushed between the thumb and finger, and covered their surfaces with a shining metallic-looking coat. These globules sunk very rapidly in strong sulphuric acid—much more so than the melted charcoal, but not with much more rapidity than the plumbago itself, from which they had been formed.

The zinc of the deflagrator is now too far gone to enable me to prosecute this research any further at present; as soon as the zinc coils can be renewed, I shall hope to resume them, and I entertain strong hopes, especially from the new improved and much enlarged deflagrator, which you are so kind as to lead me soon to expect from Philadelphia.

April 12. Having refitted the deflagrator with new zinc coils, I have repeated the experiments related above, and have the satisfaction of stating that the results are fully confirmed and even in some respects extended. The deflagrator now acts with great energy, and in consequence I have been enabled to obtain good results when using plumbago upon *both* poles. Parallelopipeds of that substance, $\frac{1}{5}$ of an inch in diameter and one inch or two inches long, being screwed into the vices connecting the poles, on being brought into contact, transmitted the fluid with intense splendour, and became fully ignited for an inch on each side; on being withdrawn a little, the usual arch of flame was formed for half an inch or more. Indeed when the instrument is in an active state, the light emitted from the plumbago points, appears to be even more intense and rich

than from charcoal; so that they may be used with advantage in class experiments, where the principal object is to exhibit the brilliancy of the light.

On examining the pieces in this and in numerous other cases, I found them beautifully studded with numerous globules of melted plumbago. They extended from within a quarter of an inch of the point, to the distance of $\frac{1}{4}$ or $\frac{1}{3}$ of an inch all around. They were larger than before and perfectly visible to the naked eye; they exhibited all the colours before described, from perfect black to pure white, including brown, amber, and topaz colours; among the white globules, some were perfectly limpid, and could not be distinguished by the eye from portions of diamond. In different repetitions of the experiment with the plumbago points, there were some varieties in the results. In one instance only, was there a globule formed *on* the point; it would seem as if the melted spheres of plumbago as soon as formed, rolled out of the current of flame, and congealed on the contiguous parts. In every instance, the plumbago on the copper side was hollowed out into a spherical cavity, and the corresponding piece on the zinc side received an accumulation more or less considerable. In most instances, and in all when the deflagrator was very active, besides the globules of melted matter, a distinct tuft or projection was formed on the zinc pole, considerably resembling the melted charcoal described in my former communications, but apparently denser and more compact; although resembling the melted charcoal, as one variety of volcanic slag resembles another, it could be easily distinguished by an eye familiarized to the appearances. In one experiment the cavity, and all the parts of the plumbago at the copper pole were completely melted on the surface, and covered with a black enamel. The appearances were somewhat varied when specimens of plumbago from different localities were used. In some instances it burnt, and even deflagrated, being completely dissipated in brilliant scintillations; the substance was rapidly consumed and no fusion was obtained. This kind of effect occurred most distinctly when there was a plumbago piece on the copper side, and a piece of charcoal on the zinc side. I have already mentioned the curious result which is obtained when this arrangement is reversed, the charcoal on the copper, and the plumbago on the zinc side; this effect was now particularly distinct and remarkable,—the charcoal on the copper side was rapidly volatilized, a deep cavity was formed, and the charcoal taken from it was instantly accumulated upon the plumbago point, forming a most beautiful protuberance, completely distinguishable from the plumbago, and presenting, when viewed by

the microscope, a congeries of aggregated spheres, with every mark of perfect fusion and with a perfect metallic lustre. I would again recommend this arrangement when the object is to attain fine pieces of melted charcoal.

April 14.—In repeating the experiments to-day, I have obtained even finer results than before. The spheres of melted plumbago were in some instances so thickly arranged as to resemble shot lying side by side; in one case they completely covered the plumbago in the part contiguous to the point on the zinc side, and were without exception white, like minute, delicate concretions of mammillary chalcedony; among a great number there was not one of a dark colour, except that when detached by the knife they exhibited slight shades of brown at the place where they were united with the general mass of plumbago. They appeared to me to be formed by the condensation of a white vapour, which in all the experiments where an active power was employed I had observed to be exhaled between the poles and partly to pass from the copper to the zinc pole, and partly to rise vertically in an abundant fume like that of the oxide proceeding from the combustion of various metals. I mentioned this circumstance in the report of my first experiments (see vol. v. p. 112 of Silliman's Journal,) but did not then make any trial to ascertain the nature of the substance. Although its abundance rendered the idea improbable, I thought it possible that it might contain alkali derived from the charcoal. It is easily condensed by inverting a glass over the fume as it rises, when it soon renders the glass opaque with a white lining. Although there was a distinct and peculiar odour in the fume, I found that the condensed matter was tasteless, and that it did not effervesce with acids, or affect the test colours for alkalies. Besides, as it is produced apparently in greater quantity, when both poles are terminated by plumbago, it seems possible that it is white volatilized carbon, giving origin, by its condensation, in a state of greater or less purity, to the grey, white, and perhaps to the limpid globules.

The deflagrator having been refitted only at the moment when a part of this paper had already gone to the press, and the remainder is called for, I am precluded by these circumstances from trying the decisive experiment of heating this white matter by means of the solar focus in a jar of pure oxygen gas, to ascertain whether it will produce carbonic acid gas.

This trial I have this morning made upon the coloured globules obtained in former experiments; they were easily detached from the plumbago by the slightest touch from the point

point of a knife, and, when collected in a white porcelain dish, they rolled about like shot, when the vessel was turned one way and another. To detach any portions of unmelted plumbago which might adhere to them, I carefully rubbed them between my thumb and finger in the palm of my hand. I then placed them upon a fragment of Wedgwood ware, floated in a dish of mercury, and slid over them a small jar of very pure oxygen gas, whose entire freedom from carbonic acid had been fully secured by washing it with solution of caustic soda, and by subsequently testing it with recently prepared lime-water; the globules were now exposed to the solar focus from the lens mentioned volume v. page 363. It was near noon, and the sky but very slightly dimmed by vapour; although they were in the focus for nearly half an hour, they did not melt, disappear, or alter their form; it appeared, however, on examining the gas, that they had given up part of their substance to the oxygen, for carbonic acid was formed, which gave a decided precipitate with lime-water. Indeed when we consider that these globules had been formed in a heat vastly more intense than that of the solar focus, we could not reasonably expect to melt them in this manner, and they are of a character so highly vitreous, that they must necessarily waste away very slowly, even when assailed by oxygen gas. In a long continued experiment, it is presumable that they would be eventually dissipated, leaving only a residuum of iron. That they contain iron is manifest, from their being attracted by the magnet, and their colour is evidently owing to this metal. Plumbago, in its natural state, is not magnetic, but it readily becomes so by being strongly heated, although without fusion, and even the powder obtained from a black lead crucible after enduring a strong furnace heat, is magnetic. It would be interesting to know, whether the limpid globules are also magnetic; but this trial I have not yet made.

I have already stated, that the white fume mentioned above appears when points of charcoal are used. I have found that this matter collects in considerable quantities a little out of the focus of heat around the zinc pole, and occasionally exhibits the appearance of a frit of white enamel, or looks a little like pumice stone, only it has the whiteness of porcelain, graduating however into light grey, and other shades, as it recedes from the intense heat. In a few instances I obtained upon the charcoal, when this substance terminated *both* poles, distinct limpid spheres, and at other times they adhered to the frit like beads on a string. Had we not been encouraged by the remarkable facts already stated, it would appear very extravagant to ask whether this white frit and these limpid spheres could arise

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from carbon, volatilized in a white state even from charcoal itself, and condensed in a form analogous to the diamond. The rigorous and obvious experiments necessary to determine this question it is not now practicable for me to make, and I must in the mean time admit the *possibility* that alkaline and earthy impurities may have contributed to the result.

In one instance contiguous to, but a little aside from, the charcoal points,, I obtained isolated dark coloured globules of melted charcoal, analogous to those of plumbago.

The opinion which I formerly stated as to the passage of a current from the copper to the zinc pole of the deflagrator, is in my view fully confirmed. Indeed, with the protection of green glasses, my eyes are sufficiently strong to enable me to look steadily at the flame during the whole of an experiment, and I can distinctly observe matter in different forms passing to the zinc pole, and collecting there, just as we see dust or other small bodies driven along by a common wind; there is also an obvious tremor, produced in the copper pole, when the instrument is in vigorous action, and we can perceive an evident vibration produced, as if by the impulse of an elastic fluid striking against the opposite pole.

If, however, the opinion which you formerly suggested to me, and which is countenanced by many facts, that the poles of the deflagrator are reversed, the copper being positive and the zinc negative, be correct, the phenomenon, as it regards the course of the current, will accord perfectly well with the received electrical hypothesis.

The number of unmelted substances being now reduced to two, namely, the anthracite and the diamond, you will readily suppose I did not neglect to make trial of them: as, however, the diamond is an absolute nonconductor and the anthracite very little better, I cannot say I had any serious hopes of success. I have made various attempts, which have failed, and after losing two diamonds, the fragments being thrown about with a strong decrepitation, I have desisted from the attempt, having, as I conceive, a more feasible project in view.

I trust you will not consider the details of the preceding pages as being too minute, provided the subject appears to you as interesting as it does to me. The fusion of charcoal and of plumbago is sufficiently remarkable; but the evident approximation of the material of these bodies towards the condition of diamond, from which they differ so remarkably in their physical properties, affords, if I mistake not, a striking confirmation of some of our leading chemical doctrines.

XXVII. *Ex-*

XXVII. *Experiments upon Diamond, Anthracite, and Plumbago, with the compound Blowpipe: in a Letter addressed to Professor* Robert Hare, *M. D. by Professor* Silliman, *dated Yale College, April* 15, 1823.*

HAVING last year caused to be constructed an apparatus, capable of containing fifty-two gallons of gas, for the supply of your compound or oxy-hydrogen blowpipe, and capable of receiving a strong impulse from pressure, I have been intending, as soon as practicable, to subject the diamond and the anthracite to its intense heat. Although their being non-conductors would be no impediment to the action of the blowpipe flame on them, still obvious considerations have always made me consider the success of such experiments as very doubtful. I allude of course to the combustibility of these bodies, from which we might expect that they would be dissipated by a flame sustained by oxygen gas.

My first trials were made by placing small diamonds in a cavity in charcoal; but the support was in every instance so rapidly consumed, that the diamonds were speedily displaced by the current of gas. I next made a chink in a piece of solid quick lime, and crowded the diamond into it; this proved a very good support, but the effulgence of light was so dazzling, that, although through green glasses I could steadily inspect the focus, it was impossible to distinguish the diamond in the perfect solar brightness. This mode of conducting the experiment proved, however, perfectly manageable, and a large dish placed beneath secured the diamonds from being lost (an accident which I had more than once met with) when suddenly displaced by the current of gas: as, however, the support was not combustible, it remained permanent, except that it was melted in the whole region of the flame, and covered with a perfect white enamel of vitreous lime. The experiments were frequently suspended, to examine the effect on the diamonds. They were found to be rapidly consumed, wasting so fast, that it was necessary, in order to examine them, to remove them from the heat at very short intervals. They exhibited, however, marks of *incipient fusion*. My experiments were performed upon small wrought diamonds, on which there were numerous polished facets, presenting extremely sharp and well defined solid edges and angles; these edges and angles were always rounded and generally obliterated. The whole surface of the diamond lost its continuity, and its lustre was much impaired; it exhibited innumerable very minute indentations and inter-

* Silliman's Journal, vol. vi. p. 349.

intermediate and corresponding salient points; the whole presenting the appearance of having been superficially softened and indented by the current of gas, or perhaps of having had its surface unequally removed by the combustion. In various places, near the edges, the diamond was consumed, with deep indentations, and occasionally where a fragment had snapped off, by decrepitation, it disclosed a conchoidal fracture and a vitreous lustre. These results were nearly uniform in various trials; and every thing seems to indicate that, were the diamond a good conductor, it would be melted by the defiagator, and were it incombustible, a globule would be obtained by the compound blowpipe.

In one experiment, in which I used a support of plumbago, there were some interesting varieties in the phænomena. The plumbago being a conductor, the light did not accumulate as it did when the support was lime, but permitted me distinctly to see the diamond through the whole experiment. It was consumed with great rapidity; a delicate halo of blueish light, clearly distinguishable from the blowpipe flame, hovered over it; the surface appeared as if softened, numerous distinct but very minute scintillations were darted from it in every direction, and I could see the minute cavities and projections which I have mentioned forming every instant. In this experiment I gave the diamond but one heat of about a minute; but on examining it with a magnifier, I was surprised to find that only a very thin layer of the gem not much thicker than writing paper remained, the rest having been burnt.*

I subjected the anthracite of Wilkesbarre, Penn., to similar trials, and, by heating it very gradually, its decrepitation was obviated. It was consumed with almost as much rapidity as the diamond, but exhibited, during the action of the heat, an evident appearance of being superficially softened; I could also distinctly see, in the midst of the intense glare of light, very minute globules forming upon the surface. These, when examined by a magnifier, proved to be perfectly white and limpid; and the whole surface of the anthracite exhibited, like the diamond, only with more distinctness, cavities and pro-

* In the Phil. Mag. for November 1821, vol. lviii, page 386, I observe the following notice by Mr. John Murray:—" By repeatedly exposing a diamond to the action of the oxy-hydrogen blowpipe in a nidus of *magnesia*, it became as *black* as charcoal, and split into fragments which displayed the *conchoidal* fracture.

" It will be found, that this gem affixed in magnesia, soon flies off in minute fragments, exhibiting the impress of the conchoidal form.

" In lately exposing the diamond, fixed on a support of pipe-clay, to the ignited gas, I succeeded in completely *indenting* it:—examined after the experiments, it exhibited proofs of having undergone *fusion*."

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jections united by flowing lines, and covered with a black varnish, exactly like some of the volcanic slags and semi-vitrifications. The remark already made respecting the diamond appears to be equally applicable to the anthracite, i. e. that its want of conducting power is the reason why it is not melted by the deflagrator, and its combustibility is the sole obstacle to its *complete* fusion by the compound blowpipe.

I next subjected a parallelopiped of plumbago to the compound flame. It was consumed with considerable rapidity, but presented at the same time numerous globules of melted matter, clearly distinguishable by the naked eye; and when the piece was afterwards examined with a good glass, it was found richly adorned with numerous perfectly white and transparent spheres, connected also by white lines of the same matter, covering the greater part of the surface for the space of ½ an inch at and around the point, and presenting a beautiful contrast with the plumbago beneath, like that of a white enamel upon a black ground.

In subsequent trials upon pieces from various localities, foreign and domestic, (confined however to very pure specimens,) I obtained still more decided results; the white transparent globules became very numerous and as large as small shot; they scratched window glass—were tasteless—harsh when crushed between the teeth, and they were not magnetic. They very much resembled melted silex, and might be supposed to be derived from impurities in the plumbago, had not their appearance been uniform in the different varieties of that substance, whose analysis has never, I believe, presented any *combined* silex, and neither good magnifiers, nor friction of the powder between the fingers, could discover the slightest trace of any foreign substance in these specimens. Add to this, in different experiments, I obtained very numerous perfectly black globules, on the same pieces which afforded the white ones. In one instance they covered an inch in length, all around; many of them were as large as common shot; and they had all the lustre and brilliancy of the most perfect black enamel. Among them were observed, here and there, globules of the lighter coloured varieties. In one instance the entire end of the parallelopiped of plumbago was occupied by a single black globule. The dark ones were uniformly attracted by the magnet, and I think were rather more sensible to it than the plumbago which had been ignited but not melted. We know how easily, in substances containing iron, the magnetic susceptibility is changed by slight variations of temperature. I am aware, however, that the dark globules may contain more iron than the plumbago from which they were derived

rived, as the combustion of part of the carbon may have somewhat diminished the proportion of that substance. I find that the fusion of the plumbago by the compound blowpipe is by no means difficult, and the instrument being in good order, good results may be anticipated with certainty. As the press is waiting while I write, it is not in my power to determine the nature of all of these various coloured globules, and particularly to ascertain whether the abundant white globules are owing to earths combined with the plumbago, or whether they are a different form of carbon. If the former be true, it proves that no existing analysis of plumbago can be correct, and would still leave the remarkable white fume, so abundantly exhaled between the poles of the deflagrator, and so rapidly transferred from the copper to the zinc pole, entirely unaccounted for. I would add, that *for the mere fusion* of plumbago, the blowpipe is much preferable to the deflagrator, but a variety of interesting phænomena in relation to both plumbago and charcoal are exhibited by the latter and not by the former.

Postscript, April 18. *Fusion of Anthracite.*

The anthracite of Rhode-Island is thought to be very pure. Dr. William Meade (see Bruce's Journal p. 36) estimates its proportion of carbon at ninety-four per cent. This anthracite I have just succeeded in melting by the compound blowpipe. It gives large brilliant black globules, not attractable by the magnet, but in other respects not to be distinguished from the dark globules of melted plumbago. The experiment was entirely successful in every trial, and the great number of the globules and their evident flow from, and connexion with, the entire mass, permitted no doubt as to their being really the melted anthracite.

The Kilkenny coal gave only white and transparent globules; but it seems rather difficult to impute this to impurities, since this anthracite is stated to contain ninety-seven per cent. of carbon.

I have exposed a diamond this afternoon to the solar focus in a jar of pure oxygen gas, but observed no signs of fusion; nor indeed did I expect it, but I wished to compare this old experiment with those related above.

The diamond is now the only substance which has not been perfectly melted.

I inserted a piece of plumbago into a cavity in quick lime, and succeeded in melting it down by the blow-pipe into two or three large globules, adhering into one mass, and occupying the cavity in the lime: these globules were limpid, and nothing remained of the original appearance of the plumbago except a few black points.

Additional

Additional Notice on the Fused Carbonaceous Bodies *.

If melted charcoal, plumbago and anthracite do really approximate towards the character of diamond, we ought to expect that, in consequence of fusion, there would be a diminution of conducting power, with respect both to heat and to electricity. This I find to be the fact. As soon as the point of charcoal is fused by the deflagrator, the power of the instrument is very much impeded by it; but as soon as the melted portion is removed, the remaining charcoal conducts as well as before; and so on, for any number of repetitions of the experiment, with the same pieces of charcoal.

The globules of melted plumbago are absolute non-conductors, as strictly so as the diamond. This fact is very pleasingly exhibited, when a point of prepared charcoal, connected with the zinc pole of the deflagrator, is made to touch a globule of *melted* plumbago, however small, still adhering to a parallelopiped of plumbago, in its natural state, screwed into the vice connected with the copper pole: not the minutest spark will pass; but if the charcoal point be moved ever so little aside, so as to touch the plumbago in its common state, or even that which has been ignited, without being fused, a vivid spark will instantly pass. This fact is the more remarkable, because it is equally true of the intensely black globules which are sensibly magnetic, and therefore contain iron, as of the light coloured and limpid ones, which are not attractable.

The globules of melted anthracite are also perfect non-conductors. This may appear the less remarkable, because the anthracite itself is scarcely a conductor; at least, this is the common opinion, and it certainly is strictly true, of that of Wilkesbarre and of that of Kilkenny; for, when both poles are tipped with those substances, there is only a minute spark, which is but little augmented when charcoal terminates one of the poles. But the fact is remarkably the reverse with the *Rhode-Island* anthracite; *this* conducts quite as well as plumbago, and I think even better, giving a very intense light, and bright scintillations. I have now no doubt, that the deflagrator will melt it, but have not had time to complete the trial.

If it should be said that the conducting power of the R. I. anthracite may be owing to iron, we are only the more embarrassed to account for the fact, that its black melted globules are insensible to the magnet, and are perfect non-conductors.

It will now probably not be deemed extravagant, if we conclude that our melted carbonaceous substances approximate very nearly to the condition of diamond.—*April* 23, 1823.

* Silliman's Journal, vol. vi. p. 378.

XXVIII. *Ob-*

XXVIII. *Observations on Marquis* LAPLACE'S *Communication to the Royal Academy of Sciences* "Sur l'Attraction des Sphères, et sur la Répulsion des Fluides élastiques." *By* JOHN HERAPATH, *Esq.* [Continued from p. 66.]

M. LAPLACE tells us that an air thermometer may be regarded as the true thermometer of nature, and finds that his function $\Pi(t)$ is proportional to the expansion of a given volume of gas under a constant pressure. From this it follows that his equation $P = i\varrho\Pi(t)$ becomes

$$P = i\varrho(F + 448);$$

F denoting the Fahr. temperature, and i being as with him a constant. Taking therefore for granted, what has been experimentally demonstrated over and over; namely, that vapours at all temperatures equal to and above that of their tensions, follow the same laws as gases; this equation of M. Laplace coincides with the theorem I have delivered, p. 269, Annals for October 1821, when discussing the experiments of Sharpe and Southern; for my squares of true temperature have the same ratio as M. Laplace's simple temperatures. It is likewise the same theorem that I have given, Annals for June 1822, p. 422, which Dr. Apjohn and Mr. Silvester imagined to be erroneous.

I have before mentioned that M. Laplace has in effect determined the point of absolute cold to be the same as I had; which I have lately been informed coincides likewise with the joint determination of two other French philosophers, MM. Clement and Desormes.

It is a curious fact that these two results of my theory, which have been corroborated by the subsequent inquiries of such a man as Laplace, are the identical cases which some of our English philosophers have opposed.

M. Laplace observes, "It results from equation 2," $\{\varrho c^2 = q'\Pi(t)\}$ "that the temperature remaining the same, the heat c diminishes by an increase of density; and consequently that the compression of a gas must develop caloric, in order to be brought to the same temperature, which experience confirms." It is true that the temperature is elevated or depressed by rapidly compressing or rarefying an air, but M. Laplace is too much of a philosopher to imagine that so vague and indefinite an appearance of agreement as he has here adduced can add any thing to the probability of his views. The same remark I might make on his preceding paragraph.

He immediately afterwards tells us "that a quadruple compression will express half the caloric of any mass of gas." Indeed by his 2d equation equal compressions of a given mass of gas, whether rapid or slow, will always evolve the same

quantity

quantity of caloric; and, generally, the whole caloric of any gas is reciprocally as the square root of its density. These are consequences which may be experimentally examined. We have unfortunately, however, no good experiments on this part of the subject, and therefore cannot come to a numerical comparison. But as far as experiments go, I conceive they make against the general consequences of M. Laplace's theory; namely, that the same compression however made develops the same quantity of heat. I have always understood that rapidity of compression is essential to the elevation of temperature, and that in very slow compressions no rise of temperature has been observed. This M. Laplace would account for on the supposition of insensible abstraction by the surrounding bodies; but was such the case, the temperature evolved would always be sensible by immersing the apparatus in water, and properly insulating it; for it would be absurd to suppose it to become latent, unless the water change its state.

I shall presently show that a very easy consequence of M. Laplace's theory is, that the elasticities being the same, the absolute quantities of caloric are equal in equal volumes of all gases. By his equation 2, therefore, equal and similar compressions of equal volumes of any two gases must evolve *equal* portions of caloric, provided the temperatures were at first equal. Now it is well known that the specific heats or capacities of different gases for caloric are different. Applying consequently M. Laplace's theorem, that the caloric expressed is as the difference of the square roots of the pressure, it follows that equal and like compressions of equal volumes of different gases, the primitive temperatures and pressures being also equal, develop the same part of the whole caloric of each, and therefore *unequal*, not *equal* quantities of caloric. Hence the results of the theory alone on compression are contrary to those of the theory on the same subject, applied to established facts, which would not be the case if the theory were correct.

It appears by the theory I have expounded, that the masses of the particles of the moving sides by which the compressions are effected, have an influence on the elevation of temperature. All other things being alike, the greater the masses of the particles in the compressing sides, the greater the rise of temperature by equal celerities of compression, even in the same gas. When therefore equal compressions are equally and similarly made on equal volumes of the same gas at the same temperature and elasticity, the elevations of temperature are as the masses of a particle of each compressing side directly. This

is

is to be understood when the compressing sides are individually homogeneous; and also when the compressions do not produce any decomposition, combustion, &c. in the gas.

Hence, if any gas, as oxygen, be compressed by mercury, the rise of temperature should be 27 times greater than in an equal volume of the same gas similarly compressed by water. I here take it for granted, that the experiments of Mr. Dalton are correct, from which I have (Annals for September 1821, p. 208,) computed the relative masses of particles of water and mercury; and likewise that a due allowance has been made for the temperature evolved by the condensation of the aqueous vapour in the space containing one portion of the gas. This allowance I conceive may be easily made by the data I have heretofore published.

Let philosophers try this very striking case; and, if the experiments are too delicate to define precisely the amounts of the elevations of temperature, they will, I think, at least perceive enough to show that the general consequence I have drawn of the superior rise of temperature by mercury is correct; and consequently that the result of M. Laplace's theory is erroneous, which makes equal compressions, however made, produce the same rise of temperature.

In M. Laplace's equation 1 $\{P = 2\pi HK\varrho^2 c^2\}$ P is the pressure or elasticity of the gas; 2π is double the ratio of the circumference of a circle to its diameter; "H est une constante qui dépend de la force répulsive de la chaleur, et qui semble ainsi devoir être la même pour tous les gaz;" K is the integral of $\psi s\, ds$ from 0 to ∞, s being an indefinitely small distance from the envelope; ϱ is the density or rather number of the particles of gas in a unity of space; and c is the caloric of one particle. It is evident, therefore, that the factor $2\pi HK$ is independent of the nature of the gas, and ought to be "la même pour tous les gaz." Hence we have universally

$$P = A\varrho^2 c^2,$$

A being $= 2\pi HK$, however different the gases. Now ϱ being the number of particles, and c the caloric of each, we have

$$AC^2 = A\varrho^2 c^2 = P,$$

putting C for the absolute caloric in a unity of space. In equal volumes, therefore, of all gases under equal pressures, there must be the same absolute quantity of caloric; and consequently the same specific quantity. That is, the capacities for caloric of equal volumes of all gases under equivalent pressures and temperatures are the same—a conclusion notoriously at variance with facts.

It is extraordinary that this consequence, so very obvious and absurd, should have escaped the penetration of such a mathematician

Pl

Tunnel across the River Thames.

Transversal section of the River at Rotherhithe

Scale of 0 10 20 30 40 50 60 70 80 90 100 200 300 400 500 600 Feet

304.

by J. Pinchback.

Plan of the descent into the Tunnel.

304.

Elevation of the Framing, shewing the Cells, in three of which the Workmen are represented in operation.

W. Sheldrick's Lithog.

mathematician as M. Laplace. But what I have adduced are not the only instances in which his theory and facts manifest the clearest opposition. Many others could be easily advanced, which, had this philosopher attempted to extend his researches beyond the three simple laws he has considered, could not fail to have shown him the marked sterility and insufficiency of his principles. For instance: had he tried to explain the facts in the conducting powers of gases discovered by Leslie, Dulong and Petit, Davy, &c., or the well known laws of vapours discovered by Dalton and Gay-Lussac, he would have found, besides the absurdities in capacity which I have mentioned, that his principles are not merely inadequate to explain, but repugnant to most of the phænomena; and are incapable of even a semblance of probability, without adding to the hypothetical assumptions already but too much outnumbering the phænomena expounded.

Cranford, London, Aug. 19, 1823. J. Herapath.

XXIX. *Description of a New Plan of Tunnelling, calculated for opening a Roadway under the Thames.** *By* M. J. Brunel, *Esq. C.E. F.R.S.*

To discover convenient and efficacious means for opening a spacious subterraneous communication between the shores of a great river, without occasioning any obstruction to the navigation, has long been a desideratum of considerable importance with the public, and in the estimation of scientific engineers. The difficulties which have opposed themselves to every attempt that has been hitherto made to execute a Tunnel under the bed of a river, have been so many and so formidable as to have prevented its successful termination in those instances where the attempts have been made.

To propose therefore the formation of a Tunnel after the abandonment of these several attempts, may appear somewhat presumptuous: on inquiring, however, into the causes of failure, it will be found that the chief difficulty to be overcome, lies in the inefficiency of the means hitherto employed for forming the excavation upon a large scale.

In the case of the drift-way made under the Thames at Rotherhithe in 1809, the water presented no obstacle for 930 feet; and when a great body of quicksand gave way and filled the drift, the miners soon overcame this obstruction, and were

* Proposals for the execution of this plan have been lately issued, with explanatory Plates, two of which we are enabled to give through the kindness of the author. (Plates II. and III.)

 able

able to proceed until they were stopped by a second irruption, which in a few minutes filled it. Nothing comes more satisfactorily in support of the system that is adopted here, than the result of the operations that were carried, under that circumstance, to an extent of 1011 feet, and within 130 feet from the opposite shore.

It is to be remarked, that at the second irruption, on examining the bed of the river, a hole was discovered 4 feet diameter, 9 feet deep, with the sides perpendicular;—a proof that the body of quicksand was not extensive; but what is most remarkable is, that this hole could be stopped merely by *throwing from above, clay partly in bags and other materials*: and after pumping the water out under a head of 25 feet of loose ground and 30 feet of water, the miners resumed the work, and proceeded a little further; but finding the hole at the first irruption increased, and the *filling* over the second very much sunk, the undertaking was abandoned.

The character of the plan before us consists in the mode of effecting the excavation, by removing no more earth than is to be replaced by the body of the Tunnel, retaining thereby the surrounding ground in its natural state of density and solidity.

In order so to effect an excavation 34 feet in breadth by 18 feet 6 inches in height, the author of this plan proposes to have the body of the Tunnel preceded by a strong framing of corresponding dimensions, as represented in the accompanying drawings, (Plate III.) and in the model proposed to be submitted for inspection. The object of this framing is to support the ground, not only in front of the Tunnel, but at the same time to protect the work of excavation in all directions. The body of the Tunnel, which is to be constructed in brick, is intended to be fitted close to the ground; and in proportion as the framing is moved forward, so the brick work is made to keep pace with it. But as this framing could not be forced forward all in one body, on account of the friction of its external sides against the surrounding earth, it is composed of eleven perpendicular frames which admit of being moved singly and independently of each other, in proportion as the ground is worked away in front. These several frames are provided with such mechanism as may be necessary to move them forward as well as to secure them against the brick-work, when they are stationary. It is to be observed, that six alternate frames are stationary, while the five intermediate ones are left free for the purpose of being moved forward, when required; these, in their turn, are made stationary for relieving the six alternate ones, and so on. Thus the progressive movement of the framing can be effected.

In

In order that a sufficient number of hands may be employed together, and with perfect security, each perpendicular frame is divided into three small chambers, which may properly be denominated cells. By this disposition, 33 men may be brought to operate together with mechanical uniformity, and quite independent of each other. These cells, which are open at the back, present in front against the ground a complete shield composed of small boards, which admit of being removed and replaced singly at pleasure.

It is in these cells that the work of excavation is carried on. There each individual is to operate on the surface opposed to him, as a workman would cut out a recess in a wall for the purpose of letting in a piece of framing, with this difference only, that instead of working upon the whole surface, he takes out one of the small boards at a time, cuts the ground to the depth of a few inches, and replaces the board before he proceed to the next. When he has thus gained from 3 to 6 inches over the whole surface, (an operation which it is expected may be made in all the cells nearly in the same time,) the frames are moved forward, and so much of the brick-work added to the body of the Tunnel. Thus intrenched and secure, 33 men may be made to carry on an excavation which is 630 feet superficial area, in regular order and uniform quantities, with as much facility and safety as if one drift only of 19 feet square was to be opened by one man.

The drift carried under the Thames in 1809, which was about the size of these cells, and was excavated likewise by only one man, proceeded at the rate of from 4 to 10 feet per day. In the plan now proposed, it is not intended that the progress should exceed 3 feet per day, because the work should proceed with mechanical uniformity in all the points together.

With regard to the line of operation, if we examine the nature of the ground we have to go through, we observe under the third stratum, which has been found to resist infiltrations, that the substrata to the depth of 86 feet are of a nature that present no obstacle to the progress of a Tunnel; we are informed that no water was met there. It is therefore through these substrata that it is proposed to penetrate, and to carry the line that is to cross the deep and navigable part of the river, leaving over the crown of the Tunnel a head of earth of from 12 to 17 feet in thickness quite undisturbed.

Admitting that in descending to or in ascending from that line we should come to a body of quicksand, such as that which was found within about 200 feet from the shore, it is then we should find in the combinations of the framing, before described,

scribed, the means that are necessary for effecting, upon a large scale, what is practised on a very small one, by miners when they meet with similar obstacles. Indeed, were it not for the means of security that are resorted to on many occasions, mines would inevitably be overwhelmed and lost.

Notwithstanding we may encounter obstacles that may retard the daily progress, it is with satisfaction we contemplate that every step we take tends to the performance and ultimate completion of the object; and if we consider that the body of the Tunnel must exceed the length of Waterloo Bridge, it must be admitted that, if, instead of two years, three were necessary to complete the undertaking, it would still prove to be the most economical plan practicable for opening a land communication across a navigable river.

No notice is taken here of the mode of constructing the descents or approaches into the Tunnel; because whatever form or direction it may be found necessary to adopt, it is obvious that no difficulties oppose themselves to the accomplishment of that part of the work, the expense of which is however taken into account in the estimate.

Nature of the Ground under the bed of the River at Rotherhithe, at a short distance below the place now proposed for opening a Roadway.

No.		Feet.	Inches.
1.	Stratum consisting of brown clay - -	9	0
2.	Loose gravel with a large quantity of water	26	8
3.	Blue alluvial earth inclining to clay - -	3	0
4.	Loam - - - - - - -	5	1
5.	Blue alluvial earth inclining to clay mixed with shells - - - - - -	3	9
6.	Calcareous rock, in which are imbedded gravel stones, and so hard as to resist the pick-axe, and to be broken only by wedges	7	6
7.	Light-coloured muddy shale, in which are imbedded pyrites and calcareous stones -	4	6
8.	Green sand, with gravel and a little water -	0	6
9.	Green sand - - - - - -	8	4
		68	4

XXX. *Notices respecting New Books.*

Recently published.

THE IVth Volume, Part II. of the Memoirs of the Wernerian Natural History Society has just appeared, and the following are its contents:

Sketch of the Geognosy of Part of the Coast of Northumberland,

berland. By W. C. Trevelyan, Esq.—On the Fossil Remains of Quadrupeds, &c. discovered in the Cavern at Kirkdale, in Yorkshire, and in other Cavities or Seams, in Limestone Rocks. By the Rev. George Young, A.M.—List of Birds observed in the Zetland Islands. By Lawrence Edmondston, Esq.—An Illustration of the Natural Family of Plants called Melastomaceæ. By Mr. David Don.—Examination by Chemical Re-agents of a Liquid from the Crater of Vulcano, one of the Lipari Islands. By John Murray, Esq.—Notice of Marine Deposites on the Margin of Loch Lomond. By Mr. J. Adamson.—Descriptions of the Esculent Fungi of Great Britain, with Observations. By Robert Kaye Greville, Esq.—Notice relative to the Habits of the Hyena of Southern Africa. By Dr. R. Knox.—An Account of three large Loadstones, one of which presented an unusual Line of Attraction. By John Deuchar.—Recollections of a Journey from Kandy to Caltura, by the way of Adam's Peak, made in the Year 1819, by Simon Sawers, Esq. and Mr. Henry Marshall, Surgeon.—Some Observations on the Falco chrysaëtos and F. fulvus of Authors, proving the Identity of the two supposed Species. By P. J. Selby, Esq.—Remarks on the different Opinions entertained regarding the specific Distinction, or Identity, of the Ring-tailed and Golden Eagles. By James Wilson, Esq.—On the Natural Expedients resorted to by Mark Yarwood, a Cheshire Boy, to supply the Want which he has sustained from Birth, of his Fore-Arms and Hands. By Dr. Hibbert.—Notice in regard to the Temperature of Mines. By Matthew Miller, Esq. —Remarks on some of the American Animals of the Genus Felis, particularly on the Jaguar (Felis Onca, Linn.). By Dr. Traill.—Observations on some Species of the Genus Mergus. By James Wilson, Esq.—Observations on the Sertularia Cuscuta of Ellis. By the Rev. Dr. Fleming.—Remarks on the Guanaco of South America. By Dr. Traill.—On a Reversed Species of Fusus (Fusus retroversus). By the Rev. Dr. Fleming. —Notice of a Specimen of the Larus eburneus, or Ivory Gull, shot in Zetland; and further Remarks on the Iceland Gull. By L. Edmondston, Esq.—Observations on the Formation of the various Lead-Spars. By Mr. James Braid, Surgeon.—Description of a New Species of Larus. By Dr. Traill.—Remarks on the Specific Characters of Birds. By Mr. W. Macgillivray.—Notes on the Geognosy of the Criff-Fell, Kirkbean, and the Needle's Eye, in Galloway. By Professor Jameson.—Observations on the Anatomy of the Beaver, (Castor Fiber, Linn.) considered as an Aquatic Animal. By Dr. Knox.—Speculations in regard to the Formation of Opal, Wood-stone, and Diamond. By Professor Jameson.—Map of

of Mackenzie's River. By Mr. W. F. Wenzel.—Observations on some Species of the Genus Vermiculum of Montagu. By the Rev. Dr. Fleming.—Notes in regard to Marine Shells found in the Line of the Ardrossan Canal. By Capt. Laskey.

Mémoires de la Société d'Histoire Naturelle de Paris: tome premier; 1re partie. Paris, Baudoin Frères, 1823, 4to.

Preparing for Publication.

Dendrologia Britannica; or Trees and Shrubs that will live in the open air of Britain throughout the year. A work useful to proprietors and possessors of estates, in selecting subjects for planting woods, parks and shrubberies; and also to persons who cultivate trees and shrubs for sale. By P. W. Watson, of Cottingham, near Hull.

The work will be accompanied with a critical preface, tracing up the subject, and reviewing the principal works that have appeared on it from the time of Evelyn, and containing other matter relative to botanical science, with illustrative diagrams, and particularly a carpologic concordance of the terms and definitions used by Gärtner, Mirbel, Richard, Decandolle, Desvaux and others, as applied to fruits and seeds.

ANALYSIS OF PERIODICAL WORKS ON NATURAL HISTORY.

Mr. G. B. Sowerby's Genera of Recent and Fossil Shells.

Nos. XVI. and XVII. of this work contains the following genera: Unio, two plates; Conus, two plates; Hyria; Calceola; Cypræa, two plates; Anodon, two plates; Lima, Nucula.

Curtis's Botanical Magazine. No. 437, 438.

Pl. 2406. *Banksia latifolia,* heretofore confounded with *serrata,* a tree 30 feet high, but described by Mr. Brown as a low shrub, plentiful in the marshes near Sidney, New South Wales: supposed to have flowered for the first time in this country last August, in the conservatory of E. Gray, Esq.—*Nerine pulchella,* "foliis glaucis, scapo bipedali, corollâ subdifformi, pallide subrubescente, rubro striatâ, loculis circiter 8-spermis," sometimes confounded with *humilis.*—*Scilla amœnula,* raised in the Chelsea garden from seeds sent under this name by Mr. Otto, of the Berlin garden. Redouté has two figures under the name *amœna,* of which this is supposed to be his tab. 130. *Itea virginica.*—*Ageratum strictum;* "caule erecto simplice scabro, foliis cordatis rugoso-venosis inæqualiter serratis, pedunculis coloratis:" raised from seeds sent from Nepaul.—*Pitcairnia staminea,* "foliis lineari-lanceolatis integerrimis, laciniis corollæ revolutis, staminibus corollâ longioribus," figured

figured also in Loddiges' Cabinet. It is a native of South America, and flowered last January in the stove of Messrs. Whitley and Co. Fulham.

Pl. 2412. *Vestia lycioides*, the only species at present known of this genus, referred first by Mr. Brown to the *Solaneæ*, which arrangement has been confirmed by Mr. David Don from an examination of the fruit. Native of Chili.—*Lupinus microcarpus*, "foliis digitatis, calycibus verticillatis inappendiculatis: labio superiore emarginato inferiore bifido ter breviore, leguminibus rhombeis hirsutis dispermis:" from Chili, and differs from all the other Lupines by its small 2-seeded pods. *Hyoscyamus orientalis*, indigenous in Iberia.—*Oxalis rosea*, raised by J. Walker, esq., of Southgate, from seeds from Chili, and agreeing in all respects, excepting the intensity of the colour, with the description and figure of Feuillée.—*Limonia parviflora*, "inermis, foliis bijugis: foliolis elliptico-lanceolatis integerrimis, corollis campanulatis, baccis oblato-sphæroideis obliquis." Nearly allied to *pentaphylla*. From China; cultivated in the stove of the magnificent establishment of the Horticultural Society at Chiswick.—*Acacia diffusa*: from the new country beyond the Blue Mountains, New South Wales, and belonging to the division of leafless *Acaciæ* pointed out by Mr. Brown as almost peculiar to Terra Australis.—*Calceolaria corymbosa*: this beautiful plant was also raised from seeds from Chili by Mr. Walker.

The public will not fail to notice the improved appearance of these Numbers, and to welcome the supply of interesting novelties which they afford.

Botanical Register.—Owing to the illness of the Editor, no descriptions have been given in the three last numbers of this work. We regret that we are from this cause still obliged to postpone our notices.

XXXI. *Proceedings of Learned Societies.*

ROYAL ACADEMY OF SCIENCES OF PARIS.

APRIL 14.—M. Arago communicated the results of the experiments recently made in England on the liquefaction of certain gaseous substances.

M. Magendie gave an account of a pathological observation made on a man who had lost the power of motion without being deprived of sensation, and in whom the anterior part of the spinal marrow was softened. This observation confirms M. Magendie's experiments on the distinct functions proper to the anterior and posterior origins of the nerves.

 M. Bory-

M. Bory-de-Saint-Vincent continued the reading of his Memoir on the Physical Geography of Spain.

M. Dupetit-Thouars commenced the reading of a Memoir on the Differences between Monocotyledones and Dicotyledones.

M. de la Borne, after having presented some new experiments on Voltaic electricity, stated that his results would be found in the sealed packet presented by him on the 10th of March.

Report on Steam Engines.

M. Dupin read the conclusions of the Report made by him in the name of a Commission, consisting of MM. Laplace, Prony, Ampere, Giraud, and Dupin, on the use of low and High Pressure Steam Engines, considered particularly with a view to the public safety. M. Gay-Lussac, whose opinion on the subject differed in many respects from those adopted in the Report, had requested to withdraw from the Commission. The conclusions adopted by a majority of the Academy were as follows:

"1. Two safety valves to be adapted to the boiler. One of them to be so disposed as to remain out of the reach of the workman who attends the boiler and the working of the engine; the other to be placed under his controul, in order that as occasion may require he may be able to lessen the pressure; whilst he would be unable to increase this pressure, as the valve to which he has no access would give vent to the steam at a lower pressure than that at which he might imprudently aim.

"2. We propose that the strength of every boiler should be proved by means of the hydraulic press, submitting it to a pressure four or five times greater than that which it would have to sustain in the usual work of the engine, so long as the pressure is between two and four atmospheres; and that above this term, the pressure in proving should be as many times greater than the usual pressure exerted by the steam in the work of the engine, as that pressure exceeds the simple pressure of the atmosphere.

"3. We propose that every manufacturer of steam-engines should be obliged to make known his method of proving, and every thing which can ensure the strength and safety of the engine, especially of the boiler and its appendages. The manufacturer to be obliged to make known to the authorities as well as to the public, the usual pressure at which the engines should be worked.

"4. The boilers of steam-engines in the neighbourhood of any habitation to be surrounded by a wall, in every case where the explosion would be sufficient to throw down the partition-wall between

between such habitation and the building containing the engine. It appears that in every case the distance between the surrounding wall, the thickness of the latter, and its distance from the boiler, may each be fixed at a metre.

" The Commission also proposes that an exact account should be kept by the authorities of all accidents happening to engines of each construction, and to publish this statement with an account of the causes and effects, the name of the manufactories where the accidents occurred, and the name of the maker of the engine; as being of all means the most efficacious for diminishing the number of accidents from the use of steam-engines, whether of simple, mean, or high pressure.

" The Commission concludes its report by stating, that in the examination of the important question submitted to the Academy by the Government, it set out from this principle, that every mechanical method carries with it its dangers, and for persevering in the employment of it, it is sufficient that these dangers do not exceed, notwithstanding their possibility, a very slight degree of probability."

April 21.—M. Dutrochet transmitted a memoir on some experiments on Vegetable Irritability.

M. Dumeril made a verbal report on the superb anatomical work of M. Autommarchi, published in numbers at Paris under the direction of M. Lasteyrie.

M. Coquebert le Montbret read the first part of his report, made in behalf of a Commission, on the Geological Description of Puy-en-Veley, by M. Bertrand Roux.

M. Chevreul read a memoir on the Causes of the Differences which are observed in various kinds of Soap, as regards their degree of hardness or softness, and their odour.

M. Bertin was elected to the situation of joint Professor of the School of Pharmacy of Montpellier.

CEYLON LITERARY AND AGRICULTURAL SOCIETY.

The Annual Meeting of this Society was held at the Chambers of the Judge of the Vice Admiralty Court, on the 16th instant, at which Sir Hardinge Giffard, who presided, delivered the following discourse, reviewing the proceedings of the Society since its formation, and suggesting to its members the best means of accomplishing the design of its establishment.

" Gentlemen—As we are now entering upon the third year of our Institution, it may be useful to look back upon our proceedings, and examine how far we have hitherto fulfilled the purpose of our Association.

"To do this with fairness to ourselves, we should bear in mind very clearly what that purpose was, as well as the means which we have enjoyed of carrying it into effect. If our purpose has been rational and useful, and the means accessible and adequate, we are bound to show to the world that we have not neglected the task which we have voluntarily undertaken. Our purpose, detailed at large in our preliminary paper of Association, may be expressed in very few words; it was the collection and subsequent diffusion of information concerning the civil and natural history of Ceylon. To this end we have solicited the communication of information from every person willing to furnish it; and having collected what may be offered, then will commence our further duty of selecting such as may appear sufficiently valuable for diffusion amongst the public.

"In the first part of this task, we have made a degree of progress to which I have to call your attention.

"To our able and excellent Vice President, Dr. Farrell, we owe some very valuable communications; and we must further ascribe much of the good spirit which has prevailed in the department over which he presides, to his salutary influence and example.

"Amongst our correspondents of this department, Messrs. Collier, Russell, and Hoatson are particularly entitled to our grateful recollection. The system of conchology traced by the former of these gentlemen, and founded not only on the external form, but on the internal physiology of the creatures inhabiting shells, promises to supersede all those which, depending upon appearance, often vague and transitory, left the knowledge of that beautiful department of nature in a state of confusion and uncertainty. We have also to thank this gentleman for his kindness in forming our collection of conchology; his opportunities at Trincomalee have given him advantages, in the immediate investigation of those subjects, which he has not permitted to pass unemployed.

"From Mr. Russell we have a highly useful Report upon the subject of smelting the iron at Ceylon. The extraordinary and valuable quality possessed by this metal, in being malleable immediately from the furnace, will probably attract attention amongst our manufacturers at home, to whom such a property must in many instances prove inestimable.

"In Mr. Hoatson's very full account of the Singhalese practice of Medicine, and their Materia Medica, if we do not find any thing to rival the improved state of medical knowledge in Europe, we can contemplate with some advantage the extent to which a perseverance in original error, unenlightened by the

the operations of the understanding, will carry the human mind. Their system seems to combine all the old absurdities of European ignorance upon this important topic, with an abundance of truly Indian origin.

"To our late very worthy member, Colonel Wright, we owe some very ingenious observations upon the action of the quicksilver in a barometer within the Tropics, and particularly the curious fact of its periodical rising and falling twice within 24 hours so regularly, as to afford almost an opportunity of measuring the lapse of time by this instrument.

"Professor Rask, a gentleman travelling for the purpose of science under the patronage of the King of Denmark, having been detained for some time in this island, was kind enough to become an Honorary Member of our Society. He has given to us a most elaborate and valuable Treatise upon the Construction of a General Alphabet, adapted to all the Indian dialects—a scheme which, if it could be adopted, at least with respect to printed communications, would much abridge the labours of learned men in investigating subjects connected with India.

"Our highly respected member, Mr. Lusignan, has furnished us with an accurate Observation of a late Transit of Mercury.

"In a short paper upon the *Maranta arundinacea*, or Indian Arrow-Root, Mr. Moon has pointed out the proper management of a vegetable only lately introduced into Ceylon, but promising, from its facility of growth and the simplicity with which it is rendered fit food, to add much to the comforts of its inhabitants.

"To extend the usefulness of our Institution, we have resolved to include Agriculture in the subjects to which our attention is directed. The communications in this instance have been few in addition to Mr. Moon's: we have, however, from Mr. Vanderlaan some important suggestions, and from an anonymous contributor an Essay on the Horticulture of Ceylon, which, however, present too discouraging a view of the subject to induce us to give it more extensive circulation.

"From our worthy Members, Mr. Marshall, Mr. Bennett, Mr. De Saram, and from Count Ranzow, we have received papers relating to subjects of Natural History, adding to our stock of information in that department of science.

"Our efforts towards compiling catalogues of the Natural History of Ceylon have been, to a certain degree, successful. Some (we wish we could say a majority) of the list of queries circulated with that view have been returned in a very satisfactory manner; in this we have to notice the zeal and diligence of

of some of the more intelligent natives, most particularly of the Modeliar of the Hapittegam Corle, who, in the returns from his district, has given us a very complete list of the various animals included in its Natural History.

"Through the kindness of Messrs. Armstrong and Knox, we have been enabled to commence the formation of a Museum, with a collection of the Birds of the interior of this island. We have received specimens from many quarters. Messrs. Gisborne, Backhouse, and several other gentlemen, have made contributions of this kind; and we have every reason to hope that their example will be followed by all who possess opportunities of thus furthering the purposes of science and improvement.

"Having thus reviewed our progress and sketched our present situation, allow me to express an opinion that we have not been deficient in our duty; and that with a very little exertion on the part of gentlemen in the several out-stations of this island, we may be enabled to render essential service to the general interests of science."—*Ceylon Government Gazette, Jan.* 25, 1823.

XXXII. *Intelligence and Miscellaneous Articles.*

ASTRONOMICAL INFORMATION.

OUR next number will contain a list of all the occultations of the fixed stars by the moon, that will be visible in the ensuing year (1824), calculated by M. Inghirami of Florence, for the meridian and parallel of *Greenwich*; a work which will be very interesting and useful both to the astronomer and navigator.

ON THE SOLAR ECLIPSE OF JULY 8.

Epping, August 14, 1823.

As no observations of the small Solar Eclipse, which happened on the 8th of July, have as yet appeared in your Journal, in the absence of any thing better on the subject, you may be inclined, perhaps, to devote a corner in your next number for the following remarks.

About four o'clock on the morning of the eclipse, there was much *cirrostratus* to the eastward, which in a great measure obstructed the sun's rays. This modification afterwards increasing, totally obscured the sun, and prevented my seeing the beginning of the eclipse; but at $5^h\ 29^m$ mean time the sun had so far advanced above the more dense parts of this range of clouds as to show a pretty distinct and well defined disc. The eclipse now was near the time of its greatest obscuration

to

to this part of the globe; and as the atmosphere became more bright, I was enabled, by an excellent achromatic telescope and power of 50, to observe the eclipse with great advantage, and found the appearance, &c. of this phænomenon to correspond with the type, &c. in Moore's and others of the Book Almanacks published by the Stationers' Company. At 39 minutes after five, the sun became perfectly clear of clouds, and continued so to the end of the eclipse, which took place at 5^h 46^m 10^s mean time, according to the meridian of Epping.

The rate of the clock was found by altitudes of the sun, taken before and after the eclipse with Troughton's Reflecting Circle on an horizon of oil, and the times computed for the latitude of the place of observation, which is 51° 41′ 41″·6 north I remain most respectfully,

Thos. Squire.

P.S. The appearance of this eclipse was an exception to the general rule; for it began and ended on the eastern side of the vertical circle of the place, passing through the centre of the sun. The oblique motion of the moon rendered the exact points of contact more difficult to be observed than if the obscuration had been greater.

There is an Almanack published in London, wherein the types and popular illustrations of the eclipses of this year are most egregiously incorrect. T. S.

ERUPTION OF GALOENGOENG IN JAVA.

The Government has received a detailed account of the eruption of the volcano Galoengoeng in October last. In this terrible visitation, one of the greatest misfortunes that have befallen Java within the memory of man, 4,011 persons perished; and 114 campongs were destroyed; 2,983 rice plantations totally destroyed, and 5,361 injured; the number of coffee trees destroyed amounts to 775,795; that of those which have suffered more or less to 3,871,742.

Batavia, March 22.

CAPTAIN SABINE'S EXPEDITION.

Accounts have been received of the progress of the Griper, Captain Clavering, on board of which Captain Sabine sailed from the Nore in the month of May last, for the purpose of carrying on the series of observations on the pendulum, in the high latitudes of the Polar seas. They arrived at the North Cape, after a tedious passage, the beginning of June, and proposed to remain at Hammerfest about three weeks. From that place they would go to Spitzbergen, as the second station of

of observations, and then proceed to the eastern coast of Greenland, intending to make their way to the most northern part of that unexplored coast, as far as the obstruction of permanent ice would permit the ship to pass. It is intended to land the instruments for observation at the highest point they should reach in Greenland, and afterwards to navigate down this hitherto almost unknown coast southwards. On quitting Greenland, they would visit Iceland, and then cross to Drontheim, in Norway, when a fourth series of observations would be completed, previous to their return in the month of November.

STATISTICS.

The following is the official return of births, marriages and deaths in Paris in the year 1822 :—

	Male.	Fem.	Total.
Births.—Legitimate	8,671	8,458	17,129
Illegitimate known ...	1,126	1,144	2,270
———— unknown..	3,765	3,716	7,481
	13,562	13,318	26,880

Marriages.—Young men and maids	5,933	
———— widows	329	
Widowers and maids	685	
———— widows	210	
	7,157	
Deaths.—Males unmarried	7,968	
——— married	2,755	
Widowers	914	
At the *Morgue*	203	
	——	11,850
Females unmarried	6,537	
——— married	2,597	
Widows	2,244	
At the *Morgue*	41	
	——	11,419
Total		23,269

Dead born.—Males 795, Females 626, = 1421.

CHESNUT-TREE BARK.

It is stated in the *Annales de l'Industrie* (vol. vi.), that this bark contains twice as much of the tanning principle as that of oak, and nearly twice as much colouring matter as logwood. With iron it forms an intensely black and durable ink. Its colouring matter has a stronger affinity than sumach for wool, and is not affected by air or by light.

PRODUC-

PRODUCTION OF CYANOGENE BY THE ACTION OF CARBON UPON NITRIC ACID.

The following particulars on this subject are derived from a paper "*On the Formation of Cyanogene or Prussine in some chemical Processes not heretofore noticed*," published in Silliman's Journal for January last (vol. vi. p. 149). The bulk of the communication relates to the probable formation of cyanogene in certain processes of nature, and it may constitute the basis of febrile miasmata.

"Some time since I was exhibiting to my class some experiments on the decomposition of nitric acid, and of nitrate of potash by charcoal, in relation to the subject of gunpowder. When I affused nitric acid on charcoal, there was, as is usual, a disengagement of the deutoxide of azote, and on standing, the acid became thick and brown, and to all appearance resembled artificial tannin, which we know is obtained by a similar process. It struck me as a circumstance not improbable, that besides the formation of nitrous gas and carbonic acid gas, cyanogene might be formed. It appeared to me, that whilst a portion of carbon combined with a part of the oxygen of the nitric acid, and the deutoxide of azote was disengaged, a part of the carbon might unite with a portion of azote, and thus generate cyanogene. Whether this explanation will hold good, I will not pretend to say, but it is certain that cyanogene was generated. By putting the charcoal and nitric acid into a retort, and collecting the gaseous products in Woulfe's bottles, arranged in the usual manner, the gases evolved were all, or the greater part, absorbed; that is to say, the nitrous gas was converted into nitric acid, by its union with the oxygen of the air contained in the bottles, &c. I saturated the water, thus impregnated, with potash, by which I formed a nitrate, carbonate, and cyanide of that alkali, as the latter was subsequently manifest. To this fluid I added the common sulphate and the persulphate of iron. The colour instantly changed, and became more or less *blue*, proving the existence of the perferrocyanite of iron, and consequently of cyanogene, which must have been formed by the union of carbon and azote. We may conclude then, that during the action of the nitric acid on the carbon, which caused the development of nitrous gas, a part of the azote of the acid must have combined with the carbon, and that another portion of the carbon, by uniting with the oxygen of the decomposed nitric acid, produced carbonic acid. The carbon in this case must have taken up a part of the azote, as well as a part of the oxygen.

If the carbon abstracted the *whole* of the azote from a given portion of nitric acid, the inference would be, that pure oxygen

was liberated; and if it took the oxygen, or a part of it, from the deutoxyde, already generated by the union of carbon and oxygen in the formation of carbonic acid, thereby leaving a compound of azote and oxygen in the state of nitrous gas—it must have reduced it to the protoxide, or gaseous oxide of azote, its first degree of oxydizement.

It is known that charcoal, especially when newly made, has the property of absorbing sundry gases, and particularly hydrogen. Might not the charcoal I used have contained hydrogen? If so, might not the nascent hydrogen during the action of the carbon have combined with a part of the oxygen of the nitric acid, and formed water; whilst that portion of the azote thus set at liberty, by combining with the carbon, may have formed the carburet of azote?

The existence of cyanogene, however, is indisputable, in whatever manner it may have originated. One atom of azote and two atoms of oxygen form the deutoxyde of azote, and two atoms of carbon with one atom of azote form cyanogene. I have not had leisure to repeat the experiment, in order to determine the quantity of cyanogene thus generated."

ON THE INFLAMMABILITY OF AMMONIACAL GAS. BY PROFESSOR SILLIMAN.

I have recently found that ammoniacal gas is much more inflammable than it is described to be in the books. Having filled with this gas, over mercury, some jars which were eight inches long by two and three quarters in diameter, I found on bringing a pendent candle over one whose mouth was covered with a glass plate, which was withdrawn at the moment, that the gas burned readily as it rose through the air, exhibiting a voluminous yellow flame. The reason why, in common cases, it appears nearly uninflammable, is, that it is used in very small quantities, and in narrow vessels, into which the common air can, at the moment, scarcely enter, and the gas is not sufficiently inflammable to burn (like pure hydrogen) merely at the surface of contact, at the mouth of the vessel. But if it rise through the air suddenly, in large volumes, and in its ascent strike the flame of a candle, it is then sufficiently inflammable to be seen through a large room, and forms a handsome experiment.—*Silliman's Journal*, vol. vi. p. 185.

NEW FACTS RESPECTING THE ATMOSPHERE.

Professor Zimmerman of Giessen has announced that he has ascertained that all atmospheric aqueous substances, as dew, snow, rain, and hail, contain meteoric iron combined with nickel. Rain also usually contains salt, and a new organic substance composed of hydrogen, oxygen and carbon, to which he has given the name of *pysine.*

BRITISH

BRITISH TENTHREDOS.

A young entomologist would esteem it a great favour if any of our correspondents would favour him, through the medium of the Philosophical Magazine, with the specific names and characters of the different British species of *Tenthredos*, and at the same time mention the plants, shrubs, or trees, on which the *larvæ* of each feed.

NEW SPECIES OF USNEA, FROM NEW SOUTH SHETLAND.

In Silliman's American Journal of Science and Arts, for January last, (vol. vi. No. I.) p. 104, is a letter from Dr. S. L. Mitchill of New York to Dr. Torrey of the same place, respecting a new Cryptogamic Plant found by Capt. Napier growing on the top of a rock, from which the snow had melted off, in an island of the group called New South Shetland. It would appear that this plant, and a dwarfish grass of which a few tufts were seen by Capt. Mackay, are the only vegetable productions of those islands; for Dr. Mitchill was informed, that "notwithstanding the frequency of lava and volcanic slag, and the occasional eruption of smoke from the earth in different places, the surface was generally covered with ice and snow, even during the southern summer;" and that "there is not a shrub nor a tree to be seen, nor any appearance of verdure to cheer the prospect."—The following is Dr. Torrey's account of the plant in question:

"The specimens presented to me by Dr. Mitchill, evidently belong to a species of Lichen. Several of them were covered with small tubercles very much resembling the fruit of Roccella, which, with the *habitat* of the plant, induced me to refer it to that genus. On a closer examination, however, I have no doubt of its being a species of Usnea without the proper fruit, merely having the *cephalodia* which are not uncommon in *U. florida*, &c.

It has the centre hyaline thread, so constant and important a character in this genus. According to Acharius, all the Usneæ are found exclusively on trees: so that this species, which grows on rocks, appears to form an exception to all the rest. In the *Flore Française*, however, the *U. florida* is said to occur sometimes on rocks, and the *U. articulata* on the ground.

The present species does not appear to be described in the *Synopsis Methodica Lichenum* of Acharius; I have therefore considered it as a new one, and have called it *U. fasciata*.

"USNEA *fasciata*.

"U. thallo pendulo scabriusculo tereti glauco virescente ramosissimo, ramis rectis nigro-fasciatis quasi articulatis, ramulis ultimis capillaceo-attenuatis, fibrillis lateralibus nullis, cephalodiis sparsis hemisphæricis atris."

 DESCRIP.—

Descrip.——From two to three inches long, and probably pendulous, roughened by minute papillæ. Common trunk short, about one line in diameter; branches dense, tapering to a filament at the extremities, and appearing beautifully articulated by transverse black bands; the joints rather longer than the diameter of the branches. Apothecia—none in the specimens I have seen. Cephalodia scattered, sometimes crowded and irregular.

Hab.——On the perpendicular volcanic rocks of New South Shetland.

Obs.——This species is nearly allied to *Usnea articulata* of Hoffman and the *Flore Française*, which is considered as a variety of *U. barbata* by Acharius; but the latter is distinguished by its gray colour, dichotomous branches, and ventricose joints, &c.

REMEDY FOR THE BITE OF SERPENTS.

M. Leguevel, "On the Properties of the Guaco," states that this shrub, which is a sort of climber, or pliant willow, found in the warm and temperate regions of the Viceroyalty of Santa Fé, towards the 45th degree of north latitude, not only possesses the property of neutralizing the venom of the rattlesnake, and other serpents whose bite proves fatal in the course of a few minutes, but may be used as a prophylactic, and with suche fficacy that some doses of the juice of the pounded leaves, properly administered, will render a person invulnerable to the bite of these reptiles. This plant was transported to Martinique in 1814, and there it was studied by M. Leguevel, who has described its botanical characters. He mentions several facts attested by persons of credit, and by the local authorities, which prove that persons bitten by the most venomous serpents have by the juice of the Guaco been saved from any ill consequences.

FEEDING OF ENGINE BOILERS.

Thomas Hall, engine man to the Glasgow Water Company, having remarked the waste of fire, when a steam engine stops working, has, instead of letting a constant supply of water into the boiler to compensate for the loss, recommended, that at each time the engine is stopped, water to the depth of eighteen inches above its usual level be poured in, by which, when the working is resumed, there is a sufficient supply of hot water, the steam is ready the moment it is required, and no increase of fuel to heat recently introduced fluid is necessary. He has himself put this method into practice, and it promises to be the means of a great saving.—*Liverpool Advertiser.*

CIRCULATION OF PERIODICAL WORKS.

The Government of Columbia, in one of their late laws, have gone beyond any other in facilitating the circulation of public papers, under the impression that it is a powerful means of promoting knowledge among the people. The following are the two principal articles of one of their late decrees.

"Article 1. Newspapers and periodical works, as well national as foreign, whatever may be their number and weight, shall pay no postage in the post-offices, and in the post conveyances of the Republic.

"Article 2. National pamphlets and other printed papers shall also enjoy the same exemption in the ordinary post conveyances, provided that the entire volume of the work does not exceed four ounces in weight. If, however, the package of national printed papers exceeds the above weight, it shall pay the ordinary postage to the contractor for the conveyance."

JOPLING'S APPARATUS FOR THE DESCRIPTION OF CURVES.

A most ingenious and useful apparatus for the production of curved lines has been invented by Mr. J. Jopling, 24 Somerset Street, Portman Square. We hope to notice it further in our next number. It appears likely to be highly interesting to the Mathematician, and useful in various branches of the Arts.

REMOVAL OF A BRICK HOUSE ENTIRE.

Instances of successful ingenuity like the following ought to be made generally known, for imitation.

"New York, 4th June.

"In certain improvements, by widening Maidenlane, it was necessary that the house No. 85 should be pulled down or removed a distance of $21\frac{1}{2}$ feet from its former front. The house is three stories high, 25 feet wide, and 45 in depth—has a slated roof, and is a valuable building. The project of removing it was conceived and undertaken by Mr. Simeon Brown, who has before removed about 20 buildings, some of them built partly with brick, and in some instances without disturbing the families or removing the furniture. This house, built entirely of brick, was estimated to weigh about 350 tons, and was removed, with all the chimneys, windows, doors, &c. standing. Being previously placed upon *ways*, the removal was commenced yesterday morning, and was performed by three bed-screws in the front, each of which was worked by two or three men.—What was deemed the most difficult part of the undertaking was, that the house must be raised about two feet from its former foundation: this was however done by two other screws placed underneath, which raised the building gradually

gradually in the exact ratio required. In the course of the day the building was moved about 16 feet, without the least detriment or jar; the other five feet will be finished this morning. There was so little danger manifest, that during the time the house was moving, the owner entertained about 150 persons within it with a handsome collation!! The expense of removing the building is about one fifth of its value, and there is no doubt that this plan will in future, in many instances, be adopted, and a great portion of the expense of pulling down and rebuilding be saved.

Obituary.—Lately, at Magdeburg, died the celebrated M. Carnot, aged 70. Political events brought him to take a leading part in the affairs of his country; but it is our province to notice him as a man of science only, in which character he was distinguished by his great attainments in the higher branches of the mathematics. A translation of his Reflections on the Theory of the Infinitesimal Calculus is printed in our 8th and 9th volumes; and his Essay upon Machines in general in our 30th volume, accompanied by a portrait of the author.

LIST OF NEW PATENTS.

To William Harwood Horrocks, of Portwood within Brimington, Cheshire, cotton manufacturer, for certain methods applicable to preparing, cleaning, dressing, and beaming silk warps, and also applicable to beaming other warps.—Dated 24th July 1823.—6 months allowed to enrol specification.

To Richard Gill, of Barrowdown, Rutlandshire, fellmonger and parchment manufacturer, for his method of preparing, dressing, and dyeing sheepskins and lambskins with the wool on for rugs, carriages, rooms, and other purposes.—24th July.—2 months.

To William Jeaks, of Great Russell-street, in the parish of St. George, Bloomsbury, Middlesex, for his apparatus for regulating the supply of water in steam-boilers, and other vessels for containing water or other liquids.—24th July.—6 months.

To William Davis, of Bourne, Gloucestershire, and of Leeds, Yorkshire, engineer, for certain improvements in machinery for shearing and dressing woollen and other cloths requiring such process.—24th July.—6 months.

To Henry Smart, of Berners-street, in the parish of St. Mary-le-bone, Middlesex, piano-forte manufacturer, for certain improvements in the construction of piano-fortes.—24th July.—6 months.

To Miles Turner, and Lawrence Angell, both of Whitehaven, Cumberland, soap-boilers, for their process to be used in the bleaching of linen or cotton yarn or cloth.—24th July.—2 months.

To John Jackson, of the town of Nottingham, gun-maker, for certain improvements in the construction of the locks used for the discharge of guns and other fire-arms upon the detonating principle.—29th July.

To Joseph Bower, of Hunslet in the parish of Leeds, Yorkshire, oil of vitriol manufacturer, and John Bland, of Hunslet aforesaid, steam-engine manufacturer, for their improvements in such steam-engines as condense out of the cylinder, by which improvement or invention the air-pump is rendered unnecessary.—31st July.—2 months.

To

To John Bainbridge, of Bread-street, Cheapside, London, merchant, who in consequence of a communication received by him from a foreigner resident in the United States of North America, merchant, is in possession of certain improvements upon machines for cutting, cropping or shearing wool or fur from skins; also for cropping or shearing woollen, silk, cotton, or other cloths and velvets, or any other fabric or fabrics thereof respectively, whether made or composed entirely of wool, silk, cotton, or other materials of which cloth or velvet is made, or of any mixture or mixtures thereof respectively; and also for the purpose of shaving pelts or skins.—31st July.—6 months.

To Louis John Pouchee, of King-street, Covent Garden, Middlesex, type founder, who, in consequence of a communication made to him by a certain foreigner residing abroad, is in possession of certain machinery or apparatus to be employed in the casting of metal types.—5th Aug.—6 months.

To Robert Dickinson, of Park-street, Southwark, Surry, esq. for his improvement in addition to the shoeing or stopping and treatment of horses' feet.—5th Aug.—6 months.

To James Barron, of Wells-street, in the parish of St. Mary-le-bone, Venetian-blind manufacturer, and Jacob Wilson, of Welbeck-street, in the parish of Mary-le-bone, upholsterer, both in the county of Middlesex, for certain improvements in the construction and manufacturing of window blinds.—11th Aug.—6 months.

To William Wigston, of Derby, engineer, for certain improvements on steam-engines.—11th Aug.—6 months.

To Henry Constantine Jennings, of Devonshire-street, in the parish of St. Mary-le-bone, Middlesex, esq. for an instrument or machine for preventing the improper escape of gas and the danger and nuisance consequent thereon.—14th Aug.—6 months.

To Robert Rogers, of New Hampshire, in the United States of America, but now of Liverpool, Lancashire, master mariner and ship-owner, for his improved lanyard for the shrouds and other rigging of ships and other vessels, and an apparatus for setting up the same.—18th Aug.—2 months.

To John Malam, of Wakefield, Yorkshire, engineer, for his mode of applying certain materials hitherto unused for that purpose to the constructing of retorts and improvements in other parts of gas apparatus.—18th Aug.—6 months.

To Thomas Leach, of Friday-street, London, merchant, but now residing at Litchfield, Staffordshire, for his improvements in certain parts of the machinery for roving, spinning, and doubling wool, cotton, silk, flax, and all other fibrous substances.—18th Aug.—6 months.

To Robert Higgins, of the city of Norwich, shawl-manufacturer, for his improved method of consuming or destroying smoke.—18th Aug.—6 mo.

To George Diggles, of College-street, in the parish of St. John, Westminster, Middlesex, for his improved bit for riding horses, and in single and double harness.—19th Aug.—6 months.

To Edward Elwell, of Wednesbury Forge, Staffordshire, spade and edge tool maker, for certain improvements in the manufacture of spades and shovels.—20th Aug.—2 months.

To Matthias Archibald Robinson, of Red Lion-street, in the parish of St. George the Martyr, Middlesex, grocer, for certain improvements in the mode of preparing the vegetable matter commonly called pearl barley, and grits or groats made from the corns of barley and oats, by which material when so prepared a superior mucilaginous beverage may be produced in a few minutes.—20th Aug.—6 months.

To John Goode, of Tottenham, Middlesex, engineer, for certain improvements in machinery, tools, or apparatus, for boring the earth for the purpose of obtaining and raising water.—20th Aug.—2 months.

METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr.* BURNEY *at Gosport; Mr.* CARY *in London, and Mr.* VEALL *at Boston.*

Gosport, at half-past Eight o'Clock, A.M.								Clouds.							Height of Barometer, in Inches, &c.		Thermometer.				Weather.	
																	London.			Boston. 8½ A.M.		
Days of Month, 1823.	Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Cirrus.	Cirrocumulus.	Cirrostr.	Stratus.	Cumulus.	Cumulostratus.	Nimbus.	Lond. 1 P.M.	Bost. 8½ A.M.	8 A.M.	Noon.	11 P.M.		London.	Boston.
July 26	29·67	60	...	66	W.	...	0·065	1	1	1	...	1	1	1	29·69	29·35	57	66	57	58·5	Fair	Cloudy
27	29·87	57	...	52	NW.	...	·200	...	...	1	...	1	1	1	29·95	29·50	55	64	60	54	Fair	Rain
28	29·88	60	...	86	SW.	0·20	·015	1	1	1	...	...	1	1	29·94	29·50	57	68	60	58	Cloudy	Cloudy
29	29·84	61	...	70	NW.	...	·020	...	...	1	...	...	1	1	29·90	29·45	60	64	60	60	Foggy	Cloudy
☾ 30	29·82	63	...	55	SW.	...	......	1	1	1	...	1	1	...	29·89	29·40	60	67	58	62·5	Fair	Cloudy
31	29·96	60	51¾	53	W.	·20	·080	1	1	1	...	1	1	1	29·99	29·50	58	69	58	60·5	Fair	Fine
August 1	30·14	63	...	51	SW.	...	·080	1	1	1	...	1	1	1	30·17	29·72	59	68	57	60	Fair	Cloudy—rain at nt.
2	30·05	62	...	70	S.	...	·070	...	...	1	...	...	1	1	30·05	29·60	57	68	63	62	Fair	Fine
3	29·90	65	...	76	SW.	·30	·620	1	...	1	...	...	...	1	29·85	29·47	63	65	61	62·5	Rain	Cloudy—rain *a.m.*
4	29·70	64	...	66	SW.	...	......	1	1	1	...	1	1	...	29·83	29·20	60	70	57	61	Fair	Cloudy—briskwind
5	29·87	62	...	53	W.	...	·260	1	1	1	...	1	1	1	29·92	29·45	60	66	52	59	Fair	Fine
● 6	29·85	62	...	56	W.	·30	·095	1	1	1	...	1	1	1	29·89	29·40	55	66	52	57·5	Fair	Cloudy
7	29·92	60	51¾	53	W.	...	·260	1	1	1	...	1	...	1	29·95	29·45	56	66	55	55	Showery	Cloudy
8	29·84	63	...	50	W.	...	·095	1	1	1	...	1	1	1	29·87	29·45	57	68	55	58	Showery	Do. showery *p.m.*
9	29·97	60	...	57	NW.	·45	·010	1	1	1	...	1	1	1	30·04	29·55	56	65	54	51	Showery	Do. rain & thun. *p.m.*
10	30·18	60	...	60	SW.	...	·020	...	...	1	...	...	...	1	30·15	29·77	56	64	62	54	Rain	Rain
11	30·10	65	...	82	SW.	...	......	1	...	1	...	...	...	1	30·10	29·57	66	74	63	64·5	Cloudy	Clou. Ther. 74·5 3 *p.m.*
12	29·98	68	...	63	S.	·20	·030	1	1	1	...	1	1	1	30·01	29·40	64	76	68	66·5	Fair	Fine 74 do.
☽ 13	29·75	65	52	56	W.	...	·145	1	1	1	...	1	1	1	29·82	29·25	67	78	62	62	Fair	Rain 75·5 4 *p.m.*
14	29·74	62	...	57	W.	...	......	...	1	1	...	1	1	...	29·80	29·05	58	64	56	58	Cloudy	Do.
15	29·88	63	...	60	SW.	·20	·055	1	...	1	...	...	1	1	29·85	29·45	56	64	52	59·5	Showery	Fine, rain at noon
16	29·60	61	...	57	SW.	...	·060	...	1	1	...	1	1	1	29·54	29·22	56	66	51	55	Showery	Fine, rain *p.m.*
17	29·90	61	...	54	W.	...	·110	1	1	1	...	1	1	1	29·98	29·50	57	66	52	57	Fair	Fine
18	29·90	61	...	61	SE.	·30	·080	...	...	1	...	...	...	1	29·99	29·60	56	64	63	58	Cloudy	Fine—rain at night
19	29·84	67	52¼	74	S.	...	·045	1	1	1	...	...	1	1	29·92	29·35	66	69	59	67·5	Rain	Cloudy—heavy rain
20	29·90	62	...	63	SW.	...	......	1	...	1	...	1	1	1	29·95	29·45	59	66	55	56	Fair	Fine—rain *p.m.*
○ 21	29·90	58	...	60	W.	·25	·285	1	...	1	...	1	1	1	29·94	29·45	56	68	56	56·5	Fair	Do.
22	29·80	61	...	80	S.	...	·220	...	...	1	...	...	...	1	29·79	29·54	57	66	60	55	Rain	Fine—rainy *p.m.*
23	29·69	64	...	74	SW.	...	·005	1	1	1	...	...	1	1	29·78	29·33	60	68	62	59	Rain	Rain
24	29·88	65	...	72	S.	...	·290	1	1	1	...	...	1	1	29·95	29·55	62	68	64	61	Showery	Cloudy, heavy rain
25	29·84	66	52½	82	NW.	·25	·170	1	1	1	...	1	1	1	29·93	29·45	63	78	71	64	Fair	Rain

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

30th SEPTEMBER 1823.

XXXIII. *On M.* Inghirami's *Lists of the Occultations of Fixed Stars. By* F. Baily, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

Many of your readers are probably acquainted with the lists of the occultations of the fixed stars by the moon, which M. Inghirami is in the habit of computing annually, for the meridian and parallel of Florence. Those lists are, from time to time, published in Baron Zach's *Correspondance Astronomique*, and also in the Milan Ephemeris: but they have not yet assumed an English dress, nor been generally circulated in this country; although (according to the explanation subjoined to the Nautical Almanac) they "afford a certain means of determining the longitude." Conceiving that such lists computed for the meridian and parallel of *Greenwich* might be useful and interesting not only to nautical persons, but also to those who have observatories here, I communicated my wishes to M. Inghirami, who, in *less than ten days*, was so obliging as to send me a list of all the occultations that will happen at Greenwich during the ensuing year (1824): and which I now forward to you for insertion in your Magazine.

The *first* column denotes the day; the *second*, the name of the star, or the constellation in which it is situated; the *third*, its magnitude; the *fourth*, the catalogue from which it is taken; where P and Z denote respectively the catalogues of Piazzi and Zach, and L, the catalogues of Lalande inserted in the *Connaissance des Tems* for the years 5, 7, 8, 9, 10, 11, 12, 13, 14, and 15: the *fifth*, and *sixth*, the right ascension and declination of the star for the dates assigned to those catalogues: the *seventh* and *eighth*, the apparent time (at *Greenwich*) of immersion and emersion; and the *last* two, the distance of the star from the centre of the moon, at those times.

The present list contains upwards of 350 occultations: yet they are by no means the whole that may be expected. For,

 our

our present catalogues are so incomplete, that they by no means contain all the stars that lie in the moon's path. An attentive observer may therefore probably witness many occultations that are not recorded in this list. It may also be proper to remark that M. Inghirami rejects from his list of occultations, all those of the small stars, except such as take place within a few days of the *new* moon: and that, throughout the whole of each lunation, he has inserted only stars of such a magnitude as may render it probable they will be visible when close to the moon, on the day on which the occultation takes place.

In a former paper on this subject, I remarked "that occultations of fixed stars by the moon, as far as the *fourth* magnitude, are easily observable *at sea;*" but I have been since informed that occultations of stars, as low as the *seventh* magnitude, have been, under favourable circumstances, frequently observed at sea. It may therefore be worthy of the consideration of the Board of Admiralty, whether they will direct that such lists should in future be annually made for the use of navigators and others. The labour of computation cannot be very great, if we bear in mind the rapidity with which the present list was formed: the whole being computed, copied, and sent off, in less than ten days. It is true that the calculations are correct to only 2 or 3 minutes of time: but this is sufficient for the *announcement* of a phenomenon, which must vary (as to time and other circumstances) at every point of the earth's surface.

It has been urged, in opposition to the printing of such a list, that the best mode of announcement is to sweep the heavens with a telescope, in the line of the moon's path: but this can seldom be done with sufficient accuracy. We are frequently deceived, as to the true course of the moon's apparent motion, in those small distances (depending frequently upon parallax) which decide whether an occultation will or will not take place: much time is occupied in such observations, which might be more usefully employed: and the observer is frequently wearied out with such a doubtful, tedious, and oftentimes useless pursuit. Moreover, not knowing the precise point to which he should direct his telescope, he may, under some circumstances (such as during twilight or thin flying clouds), miss the observation entirely.

It sometimes happens that the light of the moon will prevent the star (particularly if it be of small magnitude) from being distinctly visible. This inconvenience may in a great measure be obviated by placing an opaque body in the *focus* of the object-glass, so as to cover nearly half the field of view;

and

and in such a position that the luminous part of the moon may be always kept out of sight.

The present list contains the occultations for 1824 only: but I think it proper to state that I have also received the list for the first half of the year 1825; with an offer of those for the remainder of that year, and for the subsequent years, if I think they may be of any service to the cause of astronomy. The zeal and interest which M. Inghirami has always shown in this science, are well known to most of your astronomical readers; and entitle him to their warmest thanks. It is in the formation and calculation of these and other useful tables that he exercises the talent and ingenuity of the pupils of the *School of Astronomy* at Florence, over which he so honourably presides. I hope, before long, to be able to obtain a copy of the *general* tables, from which these annual computations are made.

I am, gentlemen, your obedient servant,

Gray's Inn, Sept. 26, 1823. FRANCIS BAILY.

[We have not room in *this* Number for more than the first *four months* of the list above mentioned: the rest shall follow in succession.—EDIT.]

List of Occultations for the Year 1824, *computed for the Meridian and Parallel of* Greenwich.

Day.	Star.	Mag.	Cat.	Æ	D	Im.	Em.	Dist. Im.	Dist. Em.
				JANUARY.					
				° ′	° ′	h m	h m	′	′
4	Caprio.	7	L 13	321 30	12 23 S	7 43	8 4	14 N	10 N
5	θ Aquarii	4·5	P 44	331 34	8 46	3 44	4 24	6 S	4 S
7	19 Piscium	6	P 182	354 3	2 23 N	5 59	6 55	14 N	1 N
8	———	7	L 8	7 35	8 12	11 46	cont.		
11	Arietis	7·8	L 11	43 3	20 39	4 14	5 14	0	9 S
—	———	7	L 8	44 30	20 56	7 51	cont.		
—	———	7	L 8	44 39	21 5	7 52	8 14	7 S	13 S
—	———	7	L 8	44 49	21 22	8 14	9 21	7 N	0
12	36 Tauri	6·7	P 236	58 23	21 27	4 20	5 19	2 N	2 S
13	118 ———	7	P 98	79 15	24 58	14 7	14 56	5 S	1 S
14	5 Gemin.	7	P 350	89 50	24 27	3 35	4 21	1 N	0
—	———	7	L 9	96 16	23 41	15 24	15 59	15 S	1 N
15	δ ———	3·4	P 57	107 2	22 20	4 51	5 28	9 N	12 N
—	———	7·8	P 97	108 45	21 55	7 28	8 25	4 N	1 N
—	63 ———	6	P 101	108 58	21 50	7 52	8 48	7 N	2 S
—	29 ———	7	P 192	113 21	20 47	16 11	16 50	6 N	11 N
17	ξ Leonis	5	P 106	140 17	12 11	8 52	9 50	7 S	7 N
—	———	7	L 8	141 52	11 43	12 15	12 30	13 N	16 N
—	ο ———	4	P 151	142 37	10 48	14 16	14 41	16 S	4 S
18	Sex n.	6	L 8	153 5	6 45	6 54	7 39	1 N	12 N
—	32 ———	7	P 98	155 29	5 40	11 8	12 4	0	14 N
—	36 ———	6	P 147	158 43	3 32	18 46	19 43	6 S	9 N

Day.	Star.	Mag.	Cat.	Æ	D	Im.	Em.	Dist. Im.	Dist. Em.
				° ′	° ′	h m	h m	′	′
22	Solitar.	9	P 298	209 2	18 19 S	15 28	15 45	16 S	7 S
—	———	8·9	P 310	209 30	18 17	16 6	17 18	5 S	9 N
23	———	7·8	L 10	221 40	21 33	14 15	15 8	1 N	12 N
—	———	7	L 10	223 17	22 11	18 9	19 28	3 N	12 N
—	———	7	P 262	223 39	22 32	18 50	20 0	9 N	1 S
24	a^1 Scorpii	5	P 189	235 24	24 43	15 8	15 54	14 S	7 S
—	———	6	L 13	235 28	24 36	15 23	16 28	7 N	1 S
—	a^2 ———	6	P 195	235 40	24 38	15 29	16 35	6 S	2 N
25	———	7·8	L 13	249 18	26 21	16 48	cont.		
28	Sagitt.	7·8	L 13	289 57	23 10	18 43	19 47	3 N	7 N
29	σ Capric.	5·6	P 67	301 58	19 44	18 49	cont.		
					FEBRUARY.				
				° ′	° ′	h m	h m	′	′
3	Piscium	9	P 119	351 2	0 54 N	5 36	6 34	13 N	0
—	16 ———	6	P 132	351 33	1 0	6 47	7 6	10 N	14 N
4	45 ———	6	P 65	3 51	6 35	9 30	10 4	7 S	14 S
7	ε Arietis	5	P 224	41 57	20 32	11 37	12 34	8 N	5 N
8	d Pleiad.	5	P 144	53 37	23 19	7 25	cont.		
—	s ———	7·8	P 156	54 16	23 14	8 17	9 12	10 S	6 S
—	f ———	5	P 157	54 19	23 26	8 51	cont.		
—	———	8·9	P 161	54 20	23 16	8 29	9 21	11 N	8 N
—	———	7·8	P 163	54 28	23 5	8 36	9 37	0	4 S
—	———	8	P 165	54 32	23 14	8 44	9 44	7 S	4 S
—	———	8·9	P 172	54 45	23 21	9 22	10 5	12 N	9 N
—	Tauri	7	L 13	56 28	23 28	12 43	13 12	13 N	13 N
9	———	7	L 9	70 11	25 0	10 16	10 47	12 N	13 N
—	k ———	6	P 247	71 29	24 44	12 4	12 59	4 S	2 S
10	———	8	Z 375	86 35	24 34	10 55	11 34	13 S	10 S
—	5 Gemin.	7	P 350	89 49	24 27	16 4	16 25	13 N	15 N
11	ω^2 ———	6·7	P 317	103 19	22 55	11 53	12 46	4 N	9 S
12	85 ———	6·7	P 246	116 0	20 24	5 38	6 27	6 N	12 N
—	Cancri	7	P 14	120 41	18 16	15 31	cont.		
—	———	8	P 20	121 3	18 10	16 1	cont.		
13	———	8·9	P 240	133 3	14 58	8 54	9 57	6 N	8 S
—	———	8	L 13	136 52	13 12	16 58	17 37	4 S	14 S
14	10 Sextan.	6	P 212	146 27	9 52	6 41	6 56	10 S	16 S
—	11 ———	6	P 218	146 53	9 16	7 40	8 23	15 N	5 N
—	π Leonis	4·5	P 225	147 24	9 0	8 46	9 32	15 N	5 S
15	———	7	L 10	160 29	3 14	7 5	8 0	10 S	4 N
—	———	7	L 13	162 0	2 51	10 12	cont.		
18	Virginis	6·7	L 8	202 10	15 22 S	10 52	11 22	1 N	8 N
22	Sagitt.	6·7	P 117	259 50	26 6	17 31	18 25	14 N	14 N
—	———	7	L 13	259 38	26 5	17 47	18 14	14 N	14 N
23	λ ———	4	P 66	273 54	25 31	19 17	20 42	4 S	0
					MARCH.				
				° ′	° ′	h m	h m	′	′
1	Piscium	6·7	P 68	348 19	0 48 S	6 7	cont.		
5	Arietis	7	L 8	36 43	18 49 N	8 12	9 4	3 N	9 N
—	μ ———	6	P 153	37 47	19 9	9 57	10 47	3 S	2 N
6	66 ———	6·7	P 65	49 12	22 6	5 32	6 35	9 S	3 S
—	Tauri	7	Z 127	51 18	22 28	10 17	cont.		

Day.

Day.	Star.	Mag.	Cat.	Æ	D	Im.	Em.	Dist. Im.	Dist. Em.
				° ′	° ′	h m	h m	′	′
6	Tauri	6·7	L 8	51 36	21 58 N	10 52	cont.		
8	118 ———	7	P 98	79 15	24 58	5 1	6 4	7 N	6 N
—	———	7·8	P 214	83 55	24 36	13 31	14 11	10 N	12 N
—	132 ———	5	P 223	84 11	24 29	14 0	14 39	4 N	8 N
9	Gemin.	7	L 9	96 16	23 41	8 30	9 35	7 S	0
—	———	7	L 9	98 4	23 1	12 24	cont.		
—	———	6·7	L 9	98 21	23 34	13 15	cont.		
10	———	7	P 224	115 4	19 49	15 32	16 17	13 N	3 N
12	ξ Leonis	5	P 106	140 17	12 11	7 14	8 16	3 S	10 N
—	o ———	4	P 151	142 37	10 48	12 36	13 35	11 S	3 N
13	32 Sextan.	7	P 98	155 29	5 41	9 55	10 35	10 N	16 N
—	36 ———	6	P 147	158 43	3 32	16 58	17 50	9 S	5 N
16	Virginis	7·8	L 10	195 28	12 41 S	8 50	9 49	10 S	5 N
17	Solitar.	7	L 10	211 27	18 59	14 40	15 9	16 S	10 S
18	———	7·8	L 10	224 11	22 15	11 52	12 9	15 S	12 S
19	Scorpii	7·8	L 13	240 30	24 56	16 31	17 32	6 N	10 N
—	———	7·8	P 14	240 42	24 57	16 38	17 39	7 N	10 N
23	Sagitt.	8	L 13	293 31	22 1	14 54	15 59	1 S	6 S
28	15 Piscium	7	P 127	351 19	0 13 N	16 35	17 4	2 N	14 N
—	16 ———	6	P 132	351 33	1 0	17 37	17 55	14 S	10 S

APRIL.

Day.	Star.	Mag.	Cat.	Æ	D	Im.	Em.	Dist. Im.	Dist. Em.
				° ′	° ′	h m	h m		
2	Arietis	7·8	L 11	48 6	21 17 N	9 59	10 45	0′	4′ N
3	Tauri	7·8	L 13	61 39	23 31	8 48	9 39	4 S	0
5	Gemin.	8	P 13	90 29	24 2	6 53	7 45	7 S	1 S
—	8 ———	7	P 30	91 1	24 1	7 48	8 46	2 S	5 N
—	9 ———	7	P 33	91 12	23 48	8 19	9 2	12 S	7 S
—	———	8	Z 393	91 33	23 40	9 4	9 37	14 S	11 S
—	10 ———	7·8	P 51	91 41	23 40	9 14	9 53	13 S	9 S
—	———	8	P 67	92 24	23 50	10 21	11 4	5 N	11 N
—	———	7	P 87	93 20	23 32	11 46	12 35	2 S	4 N
—	———	7	P 89	93 21	23 25	11 51	12 36	1 S	5 N
—	———	7	L 9	93 7	23 48	11 58	cont.		
7	Cancri	7	P 14	120 41	18 16	7 34	8 7	16 S	11 S
—	———	8	P 20	121 3	18 10	8 14	9 1	14 S	5 S
10	Leonis	6·7	L 8	165 58	0 8 S	15 15	15 58	14 S	3 S
13	Virginis	6·7	L 8	202 10	15 22	7 11	7 51	16 S	8 S
14	Libræ	7	P 171	218 58	20 29	13 12	cont.		
—	Solitar.	7·8	L 10	221 21	21 17	18 25	19 14	5 N	10 N
15	Scorpii	6	L 13	232 58	23 43	12 40	13 47	2 S	6 N
—	———	6	P 191	235 30	23 55	18 27	cont.		
16	———	7·8	L 13	246 45	25 38	12 9	12 22	15 S	14 S
18	Sagittar.	7·8	L 13	274 29	25 1	11 34	12 31	5 S	5 S
19	———	6·7	L 13	288 0	22 58	12 28	13 23	4 S	8 S
—	———	6·7	L 13	288 17	22 51	13 0	14 6	1 N	3 S
20	Capric.	8	P 33	300 47	19 48	12 58	13 59	5 N	2 S
—	———	8	L 13	300 53	19 32	14 5	cont.		
—	σ ———	5·6	P 67	301 58	19 44	15 52	16 28	10 S	15 S
30	Tauri	7·8	L 13	56 39	23 1 N	7 27	cont.		
—	———	7·8	P 213	57 16	22 38	7 50	8 36	3 S	4 S

XXXIV. *On*

XXXIV. *On Cadmium.* *By* William Herapath, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Bristol, 56 Old Market Street, August 16, 1823.

I PUBLISHED a paper in the Annals of Philosophy for June, 1822, on this new metal, in which I promised, that if any thing peculiar presented itself in my examination, I would communicate it to the public. I did not then prosecute the inquiry as much as I expected; but my attention has been drawn to this among other metals lately, and I hasten to give you some of the results; as, notwithstanding their insignificance, they will tend to place the pure metal in the hands of English chemists in greater quantity than it has hitherto been procured.

The process for obtaining pure cadmium recommended by Stromeyer is troublesome, and requires a considerable quantity of carbonate of ammonia for the re-solution of zinc and copper, besides which, if a little iron was contained in the substance, it would be left with the cadmium; as the sulphate of peroxide is precipitated by sulphuric hydrogen, dissolved by muriatic acid, and is not redissolved by excess of carbonate of ammonia.

That ingenious chemist Dr. Wollaston has also proposed a mode which is faulty; inasmuch as he directs iron to be placed in the solution to precipitate any metals which have an inferior affinity; and then zinc to be used for the same purpose; but if iron throws down any thing, a corresponding portion of itself must be dissolved, and this portion would be found with the cadmium, whether in the state of carbonate or oxide. This process would be rendered perfect by subliming the last product in the way detailed below, which is the most simple method I have yet found, and furnishes the purest metal.

In the paper above mentioned I described a powder found in the zinc works: if this be introduced into an iron bottle and tube, (similar to that used for obtaining oxygen gas from peroxide of manganese,) a piece of paper pushed down upon it, and the apparatus placed above the neck in any furnace or fire-place, where a bright red heat can be produced, the cadmium will be found in the cold part of the tube, or resting on the charred paper, if a larger quantity has sublimed than can support itself. This is very nearly pure; if not sufficiently so, the process should be repeated. It now exists as small globules, adhering to the sides of the subliming vessel; it may be reduced to a button in the way recommended in the Annals of Philosophy, June 1822, p. 436.

It is necessary to have a small quantity of some substance, such

such as wax, oil, paper, &c. to destroy the oxygen of the atmosphere in which the sublimation takes place, otherwise the cadmium is found as a brown oxide. Paper is the best, because it prevents the metal from falling amongst that from which it has been separated. All the cadmium is not yet out of the powder; to procure the remainder, dissolve in muriatic acid and precipitate with a plate of zinc: this precipitate, which consists of iron and cadmium, may be submitted to the subliming process; or if a pure salt of the metal is required, it may be dissolved in nitric acid and evaporated to dryness in the proper way to peroxidize the iron, which remains behind when the nitrate of cadmium is dissolved in water.

Another property of the pure metal, besides those published by Stromeyer and myself, is, that it makes a cracking noise like tin when bent; and, although not to the same extent, yet, from its general similarity to that metal, it is likely to be mistaken for it by those unacquainted with chemistry: it may therefore be advisable to have a ready test, by which they may be distinguished. If a piece of tin be placed in nitric acid, it is quickly converted into white oxide, but *not dissolved;* whereas the *solution* of cadmium goes on with *rapidity* in the same acid.

The late Professor Clarke was considerably embarrassed in consequence of operating upon precipitates which contained other metals; in the mode above laid down, in which its volatility is made use of to purify it, there is very little danger of not succeeding; but as it is *possible* that accident may introduce a minute quantity of extraneous matter, I shall point out modes by which it may be detected. When cadmium is perfectly pure, if the button is cut through with wire-nippers, it cuts soft like lead, and leaves a sharp edge: but if not pure, it offers considerable resistance, the nippers get through it with a snap, and the edge left is rough, from the metal breaking before the instrument has completed the cut. This is almost as good a test of its freedom from zinc (the metal it is most frequently alloyed with) as the following: dissolve a little of the metal in nitric or muriatic acid to saturation, drop in chromate of potash: if it contains the least particle of zinc, a yellow precipitate will appear, but none at all if it is pure.

Messrs. P. George and Co., zinc smelters, of Bristol, have kindly thrown every facility in my way; and, anxious for the extension of useful knowledge, they mean, if possible, to reduce a quantity to the metallic state. As a sample of its utility in the arts, I am induced to think that its sulphuret (not oxide, as erroneously stated by Dr. Clarke) would produce a pigment but little, if at all, inferior in beauty to chromate of lead.

While

While examining the products of the above gentlemen's manufactory, I met with a substance adhering to the interior of the tube leading from the retort, which is interesting from its being an alloy of 92·6 of *zinc* with 7·4 of *iron*; contrary to the opinion expressed in chemical works. Its specific gravity is 7·172 at 62°; it is extremely hard and brittle: the fracture shows broad facets like zinc, but of a duller grey colour, with surfaces more rough and granular. Its situation perhaps furnishes us with a hint, of a way to form alloys of metals which are not miscible in the open furnace.

Yours very truly,
WILLIAM HERAPATH.

XXXV. *On the Transformation of Functions.* *By Mr.* PETER NICHOLSON*.

To the Editors of the Philosophical Magazine and Journal.

THE following demonstration of the method of transforming the function of an unknown quantity x into another in $x-e$, or for extracting the root of equations, though not different in principle from that which I published in your Magazine, September 1822, will perhaps be found to be not only more explicit, but much more comprehensive, and better adapted to the understandings of the general mass of readers.

I shall therefore feel obliged by your publishing my endeavours to improve a subject so very interesting in mathematical science. And at the same time I shall observe, that the method of transforming a function, or of extracting the root of an equation, is only a branch of a more general theorem belonging to a new species of analysis, which I hope will shortly appear through the medium of your valuable Journal.

5 Claremont-place, Judd-street. P. NICHOLSON.

Proposition. *Theorem.*

If

$$\begin{aligned} x \;\ldots\ldots\ldots &= v+e \\ Ax+B \;\ldots\ldots &= Av+B_1 \\ Ax^2+Bx+C \;\ldots &= Av^2+B_2v+C_2 \\ Ax^3+Bx^2+Cx+D &= Av^3+B_3v^2+C_3v+D \\ &\&c. \end{aligned}$$

Then will

$$\begin{array}{l|l|l|l} B_1=B+eA & & & \\ B_2=B_1+eA & C_2=C+eB_1 & & \\ B_3=B_2+eA & C_3=C_2+eB_2 & D_3=D+eC_2 & \\ \&c. & \&c. & \&c. & \&c. \end{array}$$

* Communicated by the Author.

De-

Demonstration.

First let $A = A$

(1) Multiply the first side by x and the second side by $v+e$, and

$$Ax = \left\{\begin{array}{l} Av \\ \quad\quad + Ae \end{array}\right.$$

Add B to each side, and

$$Ax + B = \left\{\begin{array}{l} Av + \ \ B \\ \quad\quad + eA \end{array}\right.$$

Let $B_1 = eA + B$, and

$$Ax + B = Av + B_1$$

(2) Multiply the first side by x and the second side by $v+e$, and

$$Ax^2 + Bx = \left\{\begin{array}{l} Av^2 + B_1v \\ \quad\quad + eAv + eB_1 \end{array}\right.$$

Add C to each side, and

$$Ax^2 + Bx + C = \left\{\begin{array}{l} Av^2 + \ \ B_1v + \ \ C \\ \quad\quad + \ eAv + \ eB_1 \end{array}\right.$$

Let $B_2 = eA + B_1$, $C_2 = eB_1 + C$, and

$$Ax^2 + Bx + C = Av^2 + B_2v + C_2$$

(3) Multiply the first side by x, and the second by $v+e$, and

$$Ax^3 + Bx^2 + Cx = \left\{\begin{array}{l} Av^3 + \ \ B_2v^2 + \ \ C_2v \\ \quad\quad + eAv^2 + eB_2v + eC_2 \end{array}\right.$$

Add D to each side, and

$$Ax^3 + Bx^2 + Cx + D = \left\{\begin{array}{l} Av^3 + \ \ B_2v^2 + \ \ C_2v + \ \ D \\ \quad\quad + \ eAv^2 + eB_2v + eC_2 \end{array}\right.$$

Let $B_3 = eA + B_2$, $C_3 = eB_2 + C_2$, $D_3 = eC_2 + D$, and

$Ax^3 + Bx^2 + Cx + D = Av^3 + B_3v^2 + C_3v + D_3$: and so on.

So that having an equation of any degree, and its transformed equation, which will be of the same degree, we shall always find the transformed equation of the next higher degree by the very same steps from which the simple equation is derived from the identical equation $A = A$. Therefore from the identical equation $A = A$ we transform the function $Ax + B$ into $Av + B_1$, or derive the simple equation $Ax + B = Av + B_1$; from this simple equation we transform the function $Ax^2 + Bx + C$ into $Av^2 + {}_2B_2v + C_2$, or derive the quadratic equation $Ax^2 + Bx + C = Av^2 + B_2v + C_2$, and so on, from one degree to the next higher, till we arrive at the function required.

Therefore, without proceeding further, the principle of derivation will point out the general law. For this purpose collecting the values of the substituted quantities, we obtain

$$\begin{array}{l|l|l} B_1 = eA + B & & \\ B_2 = eA + B_1 & C_2 = eB_1 + C_1 & \\ B_3 = eA + B_2 & C_3 = eB_2 + C_2 & D_3 = eC_2 + D_2 \\ \quad \&c. & \quad \&c. & \end{array}$$

where the law of derivation for the co-efficients is obvious.

Examples.

Ex. 1. Transform the quadratic function ax^2+bx+c into another which shall have $x-e$ instead of x.

Here $A=a$, $B=b$, $C=c$; therefore $Ae=ae$

$$\left.\begin{array}{l} B_1=B+eA \\ B_2=B_1+eA \end{array}\right| \; C_2=C+eB_1.$$

Now by substituting the real quantities we shall have

$$\left.\begin{array}{l} B_1=b+ea \\ B_2=B_1+ea \end{array}\right| \; C_2=c+eB_1,$$

whence $B_2=b+2ae$, $C_2=c+be+ae^2$; therefore

$$ax^2+bx+c=a(x-e)^2+(b+2ae)(x-e)+c+be+ae^2:$$

or since $v=x-e$

$$ax^2+bx+c=av^2+(b+2ae)v+(c+be+ae^2).$$

Ex. 2. Transform the cubic function x^3-bx^2+cx-d into another in $x-e=v$.

Here $A=1$, $B=-b$, $C=c$, $D=-d$,

$$\begin{array}{l|l|l} B_1=-b+e & & \\ B_2=-b+2e & C_2=c-be+e^2 & \\ B_3=-b+3e & C_3=c-2be+3e^2 & D_3=-d+ce-be^2+e^3. \end{array}$$

Whence $x^3-bx^2+cx-d=v^3+(-b+3e)v^2+(c-2be+3e^2)v+(-d+ce-be^2+e^3)$

In the derivative table the values of B_2, B_3 are found by adding the quantity e successively. Again, by multiplying the value of B_1 by e, and adding the product to c, the third coefficient of the original equation, gives the value of C_2; and multiplying the value of B_2 by e, and adding the product to C_2, gives the value of C_3; and by multiplying the value of C_2 by e, and adding the product to $-d$, the absolute number of the given function, gives the value of D_3, the absolute number of the transformed function, which is of the same form as the original; instead of demonstrating by successive steps, as has been done, the method may be derived from two consecutive equations, as is in the following

General Theorem.

If $x-e=v$ or $x=v+e$

and $Ax^{n-1}+\ldots+K\ldots \quad =Pv^{n-1}+Qv^{n-2}+Rv^{n-3}+\ldots+\alpha,$ (A)

and $Ax^n+\ldots+Kx+L=P'v^n+Q'v^{n-1}+R'v^{n-2}+\ldots+\alpha'v+\beta',$ (B)

then will $P'=P$, $Q'=Q+eP$, $R'=R+eQ$ &c. and $\beta'=L+e\alpha$.

Demonstration.

For multiply the first side of equation (A) by x, and the second side by its equal $v+e$, and the product is the equation

Ax^n

$$Ax^n+\ldots+Kx=\left\{\begin{array}{l}Pv^n+Qv^{n-1}+Rv^{n-2}+\ldots\alpha v,\\ \quad+ePv^{n-1}+eQv^{n-2}+\ldots\ldots+e\alpha.\end{array}\right., \quad (C)$$

Add L to each side of this equation, and the sum is the equation

$$Ax^n+\ldots+Kx+L=\left\{\begin{array}{l}Pv^n+Qv^{n-1}+Rv^{n-2}+\ldots+\alpha v+L\\ \quad+ePv^{n-1}+eQv^{n-2}+\ldots\ldots\ldots+e\alpha\end{array}\right., \quad (D)$$

But since the first side of this equation (D) is the same as the first side of equation (B), and the powers of v on the second side of this the same as the powers of v on the second side of equation B, the coefficients and absolute number of the second side of this equation must be respectively equal to the coefficients and absolute number of equation B: whence

$$P'=P,\ Q'=Q+eP,\ R'=R+eQ\ \&c.\ \text{and}\ \beta'=L+e\alpha.$$

Corollary 1. Hence in any two consecutive equations, of which the first is one degree lower than the second, any coefficient of the second side of the second equation is equal to the corresponding coefficient of the first equation plus the product of the next preceding coefficient of the first equation and the quantity e.

Corollary 2. Hence if any number of equations are derived from each other, the absolute number of the second side of any equation is equal to the absolute number of the first side of the same equation plus the product of the absolute number of the second side of the next preceding equation, and the quantity e.

Corollary 3. Hence if any number of equations are derived from each other, the coefficient of the first term or highest power of v of the transformed equations is the same in all.

Hence in the following consecutive equations

$$\begin{array}{c}A=P\\ Ax+B=Pv+B_1\\ Ax^2+Bx+C=Pv^2+B_2v+C_2\\ Ax^3+Bx^2+Cx+D=Pv^3+B_3v^2+C_3v+D_3\\ \&c. \qquad\qquad \&c.\end{array}$$

By corollary 1 we derive

$$\begin{array}{c}B_2=B_1+eP\\ B_3=B_2+eP\\ \&c.\\ C_3=C_2+eB_2\\ \&c.\end{array}$$

By corollary 2 we derive

$$\begin{array}{c}B_1=B+eP\\ C_2=C+eB_1\\ D_3=D+eC_2\\ \&c.\end{array}$$

 By

By the third corollary P is the same in all, and in the first it is equal to A; therefore instead of P on the second side we have substituted A.

Hence by the following distribution the coefficients of a transformed function of any degree in $x-e$ or $x+e$ may be derived from the given coefficients A, B, C &c. of another function in x of the same degree, observing to substitute A for its equal P.

$$\begin{array}{l|l|l|l}
B_1=B+eA & & & \\
B_2=B_1+eA & C_2=C+eB_1 & & \\
B_3=B_2+eA & C_3=C_2+eB_2 & D_3=D+eC_2 & \\
\&c. & \&c. & \&c. & \&c.
\end{array}$$

The manner of proceeding with the operation of transforming the quadratic function Ax^2+Bx+C into another in $x-e$ is

$$\begin{array}{c|l|l}
A & \qquad B & \qquad C\ (e \\
\hline
 & eA+B=B_1 & \\
A & eA+B_1=B_2 & eB_1+C=C_2
\end{array}$$

Where A, B_2 are the coefficients of the first and second terms, and C_2 the absolute number of the transformed quadratic function.

Again, the manner of proceeding with the operation of transforming the cubic function Ax^3+Bx^2+Cx+D into another in $x-e$ is

$$\begin{array}{c|l|l|l}
A & \qquad B & \qquad C & \qquad D \\
\hline
 & eA+B=B_1 & & \\
 & eA+B_1=B_2 & eB_1+C=C_2 & \\
A & eA+B_2=B_3 & eB_2+C_2=C_3 & eC_2+D=D_3
\end{array}$$

Where A, B_3, C_3, D_3 are the coefficients and absolute number of the transformed function, and so on. But by making the proper substitutions in this last cubic example, we have

$$\begin{array}{l|l|l}
Ae+B=B_1 & & \\
2Ae+B=B_2 & Ae^2+Be+C=C_2 & \\
3Ae+B=B_3 & 3Ae^2+2Be+C=C_3 & Ae^3+Be^2+Ce+D=D_3
\end{array}$$

by which the original function Ax^3+Bx^2+Cx+D is transformed to

$$A(x-e)^3+(3Ae+B)(x-e)^2+(3Ae^2+2Be+C)(x-e)+(Ae^3+Be^2+Ce+D)$$

Problem.

To transform any rational function of x into another which shall have $x\pm e$ instead of x.

Supposing the powers of x to decrease uniformly by unity from left to right, A, B, C &c. to represent the coefficients and absolute number of the proposed function in the order of the powers of x, as in the demonstration.

Place

Place the coefficients A, B, C &c. in a line, and the parts of the operation to be performed in as many other parallel lines to the line of coefficients as the exponent of the highest power of x has unity, calling them the first, second, third, &c. lines, as they succeed the line of coefficients. Bring down the first coefficient A immediately below A into the last line, which will be the first coefficient of the transformed function.

Multiply the coefficient A of the highest power into e; add the product eA to the second coefficient B, and place the sum B_1 in the first line under the second coefficient B: add the product eA to B_1, and place the same B_2 under B_1; proceed in this manner until all the lines have been used.

Multiply B_1 into e; add the product eB_1 to the next coefficient C above, and write the sum C_2 in the second line in a column under C. Multiply B_2 into e; add the product eB_2 to C_2 and place the sum C_3 under C_2. Proceed to find the remaining members of the column so that one may occupy each succeeding line.

Proceed in the same manner with each succeeding column to the last line, which will contain the coefficients, and the absolute number of the transformed function in the same number of members, and in the same order as the coefficients and absolute number of the original function.

Numerical Examples.

Ex. 1. Transform the function $4x^3+15x^2-6x+15$ into another which shall have $x-3$ instead of x.

Operation.

4	+15	−6	15	(3
	12+15=27			
	12+27=39	81− 6= 75		
4	12+39=51	117+75=192	225+15=240	

So that the function $4x^3+15x^2-6x+15$ is transformed to

$$4(x-3)^3+51(x-3)^2+192(x-3)+240;$$

or if $x-3=v$

$$4x^3+15x^2-6x+15=4v^3+51v^2+192v+240.$$

But as the minor parts of the process of multiplying and adding are so easily performed mentally, the results may only be set down, which will save a great deal of writing, and render long operations much clearer; and thus the work now executed will stand thus,

$$\begin{array}{llll} 4 & +15- & 6+ & 15(3 \\ \hline & +27 & & \\ & +39+ & 75 & \\ 4 & +51+ & 192+ & 240. \end{array}$$

This

This mode of operation will be observed in the following examples.

Ex. 2. Transform the function $x^4-9x^3+27x^2-41x+30$ into another which shall have $x-4$ instead of x.

Operation.

$$\begin{array}{l}
1-9+27-41+30(4 \\
\hline
\;\;-5 \\
\;\;-1+\;7 \\
\;\;+3+\;3-13 \\
1+7+15-\;1-22
\end{array}$$

And thus the function $x^4-9x^3+27x^2-41x+30$ is transformed to $(x-4)^4+7(x-4)^3+15(x-4)^2-(x-4)-22$.

Ex. 3. Transform the function $x^4-3x^3-3x^2+15x-29$ into another function which shall have $x+3$ instead of x.

Operation.

$$\begin{array}{l}
1-\;3-\;3+\;15-29(-3 \\
\hline
\;\;-\;6 \\
\;\;-\;9+15 \\
\;\;-12+42-\;30 \\
\;\;-15+78-156+61
\end{array}$$

So that the original function $x^4-3x^3-3x^2+15x-29$ is transformed to $(x+3)^4-15(x+3)^3+78(x+3)^2-156(x+3)+61$.

Ex. 4. Transform the function $x^3-7x^2+17x-15$ into another function which shall have $x-\frac{7}{3}$ instead of x.

In this example it will be the most eligible to find equivalents to the coefficients and absolute number in the form of fractions, so that the denominators may be the regular powers of the denominator of the fraction, by which the base x of the original function is to be diminished; and then perform the operation with the numerators instead of the respective coefficients, as if whole numbers, then x by 7.

Now $7=\frac{3\cdot7}{3}=\frac{21}{3}$, $17=\frac{3^2\cdot17}{3^2}=\frac{153}{9}$, and $15=\frac{3^3\cdot15}{3^3}=\frac{405}{27}$.

Whence the operation

$$\begin{array}{l}
1-21+153-405(7 \\
\hline
\;\;-14 \\
\;\;-\;\;7+55 \\
1\mp\;\;0+\;6-20
\end{array}$$

The original function $x^3-7x^2+17x-15$ is thus transformed to $(x-\frac{7}{3})^3\pm0+\frac{6}{9}(x-\frac{7}{3})-\frac{20}{27}$.

So that in fact this process is equivalent to taking away the second term, as it is called, or to transform the equation to another which shall want the second term.

Ex.

Ex. 5. Transform the function $x^3-7x^2+17x-15$ into another which shall have $x-2{\cdot}333$ instead of x.

Operation.

1 − 7	+ 17	− 15(2·333
− 5		
− 3	+ 7	
1 − 1	+ 1	− 1
− 0·7		
− 0·4	+ 0·79	
1 − 0·1	+ 0·67	− 0·763
− 0·07		
− 0·04	+ 0·6679	
1 − 0·01	+ 0·6667	− 0·742963
− 0·007		
− 0·004	+ 0·666679	
1 − 0·001	+ 0·666667	− 0·740962963

Here $\frac{7}{3}=2{\cdot}333$ &c. and the coefficients of the transformed function are continually approaching respectively to 1, 0, $\frac{2}{3}$ and $\frac{20}{27}$.

Comparing this example and the next preceding; it appears that the length of the operation is greatly increased by reducing the vulgar fraction to a decimal.

Examples in the extraction of roots will not be necessary at this time, as these have already been given in the Philosophical Magazine for September 1822, before alluded to.

XXXVI. *On the Changes which have taken place in the Declination of some of the principal Fixed Stars.* *By* John Pond, *Esq. Astronomer Royal, F.R.S.**

THE mural circle having in September last been put into complete repair, and declared by Mr. Troughton to be in as perfect a state as when first erected, I resumed my examination of the principal fixed stars which form the Greenwich catalogue. In the course of a very short time, I found that several anomalies, which had previously given me much perplexity, still subsisted: some of these were of such a nature as to lead to a suspicion that a change might possibly have taken place in the figure of the instrument; on the other hand, there were circumstances that strongly militated against such a supposition.

Several of the stars in which the supposed discordance ap-

* From the Philosophical Transactions of the Royal Society of London, 1823. Part I.

peared

peared the greatest, passed over almost the same divisions with others, in which no such discordance could be perceived. Moreover, in examining these discordances in different points of view (that is, both with respect to their right ascensions and polar distances), I fancied I perceived something like a general law, that was quite incompatible with any possible hypothesis of error in the instrument.

On a point of this importance, I clearly saw the necessity of devising some new method of observation which might decide with certainty that which otherwise would become an endless subject of doubt and conjecture.

I had often attempted to observe the altitudes of stars by means of an artificial horizon of quicksilver, or other fluid, but had abandoned the attempt from the difficulty of protecting it from the wind, and from the number of observations I lost in fruitless experiments. To this method I had again recourse; and by means of wooden boxes of different sizes and figures, according to the different altitudes of the stars, I have sufficiently accomplished my purpose. A very few observations were sufficient to convince me that the instrument was in every respect perfect, and that I might repose the greatest confidence in every result it gave.

Several stars, and particularly those most discordant, I have observed by this new method, and find their places, without any exception, to agree within a fraction of a second with those determined by direct measurement from the pole.

Presuming that the observations* which accompany this paper will remove every shadow of a doubt as to the accuracy of the instrument, I shall now proceed to state, in as few words as possible, the nature of the changes which appear to me to have taken place since the year 1812.

If Bradley's catalogue of stars for the year 1756 be compared with the Greenwich catalogue for 1813, it will be possible to deduce the annual variation for each star for the mean period, or for the year 1784, on the supposition of uniformity in the proper motion of each star; then allowing for the change of precession for each star, a catalogue may be computed for any distant period; as for example, the present year 1822. Suppose such a catalogue computed, which I have named a predicted catalogue; then, if this be compared with the observed catalogue for the same year, the following differences will be found to subsist between them.

The general tendency of all the stars will be to appear to the south of their predicted places, and this tendency seems to be greater in southern than in northern stars: if any star

* Vide Appendix, p. 178.

be

be found north of its predicted place, it will always be a star north of the zenith, and the quantity of its motion extremely small. There may be observed a much greater tendency to southern motion in some parts of the heavens than in opposite or distant parts as to right ascension, and in much the greater portion of the heavens the southern motion seems to prevail. A southern star, as *Sirius*, situated in that part of the heavens most favourable for southern motion, will be found more to the south of its predicted place than *Antares*, situated in the part least favourable for southern motion, though it is itself more southward.

Several stars have moved more from their predicted places than other neighbouring stars: when this happens, the motion is always southward; I have yet met with no exception to this rule; not a single star can be found having an *extra* tendency to northern motion; and indeed the northern motion in any star is so very small, that it would never have excited attention.

A very great deviation will be found in three very bright stars, *Capella*, *Procyon*, and *Sirius:* the proper motion of each of these is southward; it therefore follows that these proper motions are accelerated. The proper motion of *Arcturus* is very great, and likewise southward. It is situated in that part of the heavens where the southern tendency is least discernible, and is nearly quiescent; its proper motion in polar distance may therefore be considered as uniform. There is a circumstance that deserves notice, though it may be merely accidental: the stars in the Greenwich catalogue, whose proper motions are south, nearly equal in number those that are north, yet the *quantity* of southern proper motion exceeds the northern in the proportion of four to one.

I shall at present offer no conjecture on the cause of these deviations, but endeavour, by continued observations, more accurately to ascertain the law which they follow. Should the weather prove favourable for observation, I hope before the Society separate for the summer, to be able to give greater accuracy to the numbers here subjoined. Indeed I should not have made so early a communication on the subject, but as the Greenwich observations of 1820 are about to be published, they might without this explanation have appeared erroneous; for I find that during that year the instrument was rather defective from general unsteadiness, than from any perceptible deviation of the telescope. It was not till after the month of February 1821, that the instrument got completely out of repair. It must however be admitted, that the observations of that year ought not to be employed in the deter-

mination

mination of such small quantities as form the subject of the present communication.

Horizontal point of the Circle as found by different Stars observed by direct Vision and Reflection, from 11*th to* 23*d March* 1822.

h Urs. Maj.	123° 30′	29″·55
v		28·95
m		29·75
β		29·45
α		29·50
o		29·05
Castor		29·86
Capella		29·55
Pollux		29·95
β Aurigæ . . .		29·35
Mean of 10		29·54
Sirius		29·47

There being no perceptible difference in the results obtained near the zenith and near the horizon, it may be concluded that the instrument has no deviation, either from flexion of the telescope or change of figure.

Appendix *to the preceding Paper.*

The observations which have been made during the last summer, confirm in a very decided manner the results which formed the subject of my last communication; in which I laid before the Society the nature of the differences that exist between the computed places of the principal stars of the Greenwich catalogue, and those deduced from actual observation. It is not my present intention to offer any explanation of the cause of these phænomena, although many obvious conjectures present themselves, the value of which it will require perhaps many years to determine. It is now my principal object to consider the force of that explanation of the differences in question, which will most readily occur to every astronomer, namely, that the whole may arise either from error committed by the observer, or from defect in the instruments of observation: this objection being the more weighty from the circumstance, that the observations of three distant periods are employed, and that an error in those of either period (but particularly of the two latter) would materially affect the result now under consideration.

I believe that every person, in proportion to his experience in the use of astronomical instruments (even of the most unexceptionable

exceptionable construction), will be cautious in admitting the accuracy of any results, with whatever care the observations may have been made, which appear to militate against any received theory of astronomy; and I shall have occasion myself to show, from the great discordances between instruments of the highest reputation, that this distrust is but too well founded. More particularly ought our suspicion to be excited, when such anomalies are found to exist, as bear some direct proportion to the zenith distances of the stars observed. In all such cases we should never hesitate, I think, to ascribe the anomalies to defective observation. If therefore in the present instance any part of the discordances in question can be shown to depend on polar or zenith distances, I shall willingly admit, as to such part of them at least, that they are no otherwise of importance, than as affording data for leading to the detection of some hitherto undiscovered errors. The anomalies, however, that have led me on to this inquiry, and to which alone I attach any importance, are found to depend rather on the right ascensions, than on the declinations of the stars. Accordingly I found, while collecting observations to form a catalogue for the present period, that I could more nearly predict the deviation of a star from its computed place, by knowing its right ascension, than its declination. Now it is not easy to conceive in what way the error of an instrument for measuring declination, fixed in the meridian, can be occasioned by any circumstance depending on the right ascension of a star to be observed.

The general nature of the deviation of the stars from their computed places will be best understood from the annexed tables*; in one of which the principal stars of the Greenwich catalogue are arranged according to north polar distance, and in the other, in the order of their right ascensions.

From these tables it will appear, according to my statement in the former part of this paper, that the general tendency of the deviation is towards the south; that in about one-third part of the heavens in right ascension this southern tendency is very inconsiderable, and would hardly have excited attention: for in this part, stars between the zenith and the pole, appear a very small quantity to the northward; whereas in the remaining and most considerable portion of the heavens, every star appears to be a considerable quantity to the south of its computed place; and with few exceptions, the more

* As our limits do not enable us to insert these tables, we can only refer the reader to them in the Philosophical Transactions for 1823, Part I. page 61, &c.—Edit.

southward stars have a greater tendency to deviation than the northern ones.

If we select from the preceding tables those stars which were least frequently observed, at one or all of the three periods, we shall find that they all tend to confirm the foregoing general results; though they must be regarded as doing so rather by their united effect, than by their weight of evidence when considered singly. Stars that have been but seldom observed, give results considerably affected by accidental error of observation; which error is quite of a different nature from that produced by permanent defect in the instrument, and which repetition of observation has no tendency to remove.

If the deviations of those stars that have been imperfectly observed, were attributable either to error of observation, or defect in the instruments, the deviation would either follow no law at all, or some law depending upon zenith distance: but the facts we have seen to be at variance with either of these hypotheses. Not however to rest satisfied with these considerations, drawn from the general tendency of all the stars without exception, let us select some striking examples of deviation, in particular groups of stars, on which we might be satisfied to rest the issue of this question. Of these groups I have marked *five* in the table of stars arranged according to north-polar distance, each of which we will take the pains to consider more attentively.

1. There are six stars in my catalogue north of γ *Draconis*, of which three are found to the north, and three to the south of their computed places. These inequalities may appear at first sight to be wholly accidental; but if we pay attention to the right ascension, we shall find that the three which appear to the northward, are situated in that part of the heavens as to right ascension where the southern deviation is the least perceptible, and that the three which appear to the southward, are in that part as to right ascension where the southern deviation is the greatest. But of these six stars there are two, α *Cassiopeiæ*, and γ *Ursæ Majoris*, which deserve further consideration. These two stars are within less than one degree of each other in polar distance, and consequently pass over the meridian at nearly the same altitude. The observations of Bradley on the stars north of the zenith are not so numerous as could be wished; but each of the two stars in question was observed by him about five times towards the year 1753; that is, 60 years from the date of my catalogue of 1813. I have carefully recomputed the predicted places of these stars, and I find α *Cassiopeiæ* not less than $1''{\cdot}5$ to the south of its predicted

dicted place, and γ *Ursæ Majoris* half a second to the north. Now I am quite at a loss to conceive how this difference in so small an arc can arise from error of observation, and I can only attribute it to that cause, whatever it may be, which seems so generally to depend not on the polar distance, but on the right ascension of the star.

2. The second group which I shall consider, contains the stars α *Arietis*, *Arcturus*, and *Aldebaran*, comprehended within an arc of about six degrees and a half. Of these three, *Arcturus* alone has yet been observed by reflection; but from the present very perfect state of the Greenwich circle, which the method of reflection has enabled me to ascertain, it cannot be doubted that the places of the two other stars are well determined*. In *Arcturus* the southern deviation is nearly insensible, but in the two other stars it is very considerable, being in each not less than 1″·5. Now these three stars, but particularly the two latter, are among those that have been most assiduously observed by Bradley and myself at each of the three periods. Let us suppose then, if it be possible, that the whole of these deviations arise from error of observation: or, in other words, that no systematic deviation has really taken place in the stars, but that their proper motions are uniform. Then we must admit that the mural quadrant and the mural circle have at each period given the polar distance of *Arcturus* correct, or at least subject to the same constant error; and as this star has been observed at each period, at all times of the day, and at all seasons of the year, the observations may be considered as perfectly exempt from accidental error. It will, I believe, be readily conceded that both instruments are so far perfect, that if the error be either nothing, or a given quantity at one point of the arc, the errors must be very nearly indeed the same within a moderate distance, as within 15 degrees, for instance, of that point. Upon this supposition, how can we possibly reconcile the great errors that must have been committed in stars, adjacent as to polar distance, but of opposite right ascensions? I do not wish to press these remarks, in order to obtain greater confidence than they deserve, for observations which can never be regarded with too much suspicion; but the arguments I have used appear to me to follow logically from the data before us, and strongly to indicate the probability that some cause purely astronomical has, at least, some share in producing these unexpected deviations.

3. The third group, α *Herculis*, α *Pegasi*, and *Regulus*, is still more remarkable, being comprehended within two degrees

* This has been confirmed by subsequent observation.

of

of declination, and two of the stars, *α Herculis*, and *α Pegasi**, being within half a degree of each other. In this group *α Pegasi* is at least 3″ south of its predicted place, whereas the other two stars have not deviated much more than 0″·5 to the south.

4. *α Orionis*, *α Serpentis*, and *Procyon*, furnish an example equally striking, they being within less than 2° of declination from each other; *α Serpentis* is exactly in its predicted place, while *α Orionis* and *Procyon* are each of them at least 2″ to the south.

5. *Rigel*, *Spica Virginis*, and *Sirius*, are not contained within so short an arc as the former groups, nor are their places so well determined, on account of their proximity to the horizon; but they afford another instance of the inequality of southern deviation in stars having nearly the same polar distance, but opposite right ascensions.

But leaving the considerations suggested by these groups of stars, let us examine more minutely the different hypotheses that may be formed on the supposition, that the whole of these deviations depends on error of observation caused by some defect in the instruments employed: this investigation becomes the more necessary, as it does not appear that Dr. Brinkley, with his instrument at Dublin, has met with similar discordances. Admitting the accuracy of the observations of Bradley to form the ground-work of this inquiry, there are then two distinct hypotheses, that may be formed by those who are inclined to maintain, that the proper motions of the stars are uniform; and that the discordances in question have their source, not in any astronomical cause, but in some erroneous system of observation. Of the observations from which the catalogues of 1813 and of the present year have been computed, we may suppose the one or the other to be erroneous. Let us consider the consequences of each hypothesis.

Let us first suppose the error to be in the observations of 1813. Then the observations of 1756 and 1822 being supposed perfect, a catalogue for the year 1813 may be computed by interpolation; such a catalogue is annexed, and this (assumed to be correct) compared with the observed catalogue of 1813, will show the errors of observations at that period. On this assumption the Greenwich circle must, in 1813, have been in a very defective state; and admitting the instrument to be now perfect, this can be only attributed to the insufficiency of the braces which then connected the telescope to the circle; for this is the only difference between the instrument

* The lunar nutation of *α Pegasi* was nearly a minimum at each period.

in

in its former and in its present state. The natural tendency of any such defect would be, I think, continually to increase, and to give results every year more and more distant from the truth: but this is contrary to the known history of the Greenwich observations, which I have found gradually for some time past approaching to those results which are obtained at the present day, and which, according to our present hypothesis, are supposed to be nearly perfect. If the catalogue of 1813 were really so erroneous, as our present hypothesis would compel us to regard it, then it would appear that Dr. Brinkley's catalogue for the same period must have been still more erroneous, as may be seen by inspection of the annexed tables. Now admitting for a moment that there were at that time certain imperfections in the Greenwich and Dublin instruments, no person will believe them to have been so imperfect as our present hypothesis would tend to represent them.

Let us now examine the second hypothesis, which presumes the catalogue of 1813 to have been perfect, and consider what confidence is due to the Greenwich observations of the present day. This investigation is to be regarded as important, not merely with a view to the discussion of the nature of the discordances in question, but also from the circumstance, that instruments of well-known celebrity are represented as giving very different results; for which reason I shall be excused for entering into considerable details on this particular question. As the principal reliance I place on the accuracy of the present catalogue, and on the superiority of the Greenwich circle over all other instruments, with the history of which I am acquainted, is derived from the coincidence of the results obtained by the two independent methods; the one of direct measurement of polar distance, the other of observing the angular distance of the direct and reflected image of the stars, it becomes of some importance to consider in what way this coincidence is a proof of the accuracy of either. The source of error the most to be dreaded in every instrument whatever, quadrant or circle, is that which will be caused by the flexure of the materials of which the instrument is made. It is impossible in theory that any instrument can be wholly free from this defect. In the Greenwich circle the number of microscopes placed round its circumference have an obvious tendency to diminish this error, though they cannot annihilate it; but they have no tendency whatever to diminish the error arising from the flexure of the telescope attached to the circle.

The effect of flexure in any circle will be, in the first instance, to give an erroneous distance from the pole to the zenith: in instruments that turn in azimuth, of the usual construction,

struction, the error thus occasioned will be applied to every star under the form of co-latitude, and a star south of the zenith will be moreover affected by the probably opposite flexure due to that point of the instrument on which the star is observed. This in stars near the equator, or a little to the northward of it, will in our latitude give an error in polar distance, amounting to about double the error committed in determining the co-latitude. On the contrary, the polar distances of stars north of the zenith, being affected only by the difference of two flexures, will be more accurately determined as they approach nearer to the pole, where the errors will wholly vanish. Now, though in the usual mode of employing the Greenwich circle, viz. in measuring directly polar distance, the co-latitude does not become an object of inquiry, yet any flexure of the circle will produce a system of errors of the same nature as those above pointed out. In instruments, like that of Dublin, which turn in azimuth, and with which the observer has to find the place of all the stars by measuring the double of their zenith distances, if he does not find the same zenith point with different stars (provided the instrument be well divided) he may be sure that flexure takes place; but he cannot infer the converse, that flexure does not take place, from his obtaining with all the stars the same error in the line of collimation. For if the flexure be the same on both sides of the zenith, a supposition by no means improbable, the observer will then have no indication of flexure by the usual method of determining the error of collimation by stars of different altitudes. Let us suppose that, with an instrument liable to flexure, it is required to measure by both methods the meridional distance of any two stars. The angular distance of the direct images will (as we have already seen) be affected by the difference, or by the sum of two flexures, according as the stars are placed on the same or on opposite sides of the zenith. In viewing the reflected images, the instrument, receiving two new positions, will be subject to two new flexures, by the sum or difference of which (as it may happen) the angular distance of the reflected images will be affected.

The most probable supposition to be made concerning the flexures is, that at equal inclinations with the horizon, above and below it, they will be the same nearly both in direction and degree, and therefore that the two images below the horizon will approach by nearly the same quantity that the direct images receded, or *vice versâ*. With an instrument therefore having such a system of flexures, the double altitude of each star will be correctly ascertained; but stars of different altitudes will give different determinations of the horizontal point.

From

From observations thus obtained, a near approximation to the true angular distance might be inferred, by taking a mean between the distances of the direct and of the reflected images. The least probable supposition concerning the flexures is, that at equal inclinations above and below the horizon, they will be equal, but in opposite directions; the consequence of which would be, that the direct and reflected images would approach to or recede from one another by the same quantity: the double altitudes of each star would be incorrectly given, but every star would give the same determination of the horizontal point. To suppose however the existence of such a system of flexures, would be to suppose that gravity produced the same change of form in the instrument, as if its direction were inverted; and since the horizontal line is that at which according to the supposed system a contrary flexure will take place, the flexure at or near the horizon should be zero, where, however, according to the known laws of mechanics it ought to be the greatest. Such a system therefore must be considered as mechanically next to impossible.

If then an instrument give the angular distances both by reflection and by direct vision the same, and the same determination of the horizontal line from stars of whatever altitude, there are then only two hypotheses that can be formed respecting such an instrument; either that the flexures are insensible, or that they are such as are absolutely inconsistent with the laws of mechanics. Hence I conclude that the coincidence of the results by direct vision and by reflection, and the uniform determination of the horizontal point, will be the strongest proof of the non-flexure of the instrument, and of the accuracy of both results*.

In illustration of the whole of the preceding observations, let us examine two catalogues, those of Dr. Brinkley and Mr. Bessel, which have lately much excited the attention of astronomers. It is obvious, by merely inspecting these catalogues, a comparison of which with the Greenwich catalogue I here subjoin, that one or both of the instruments used by these astronomers must be erroneous; and it seems to me, that the source of error is the very flexure, the nature and effects of which we have been considering. For, if we attend to the differences between these two catalogues, we shall find that the six stars near the equator differ 5″ from one another, whereas the stars near the zenith do not differ above 2″·5. In which direction flexure will affect the zenith distances, is a

* I must also notice that the method by reflection possesses, in common with instruments turning in azimuth, the advantage of measuring the double of the required angle.

matter quite accidental, depending on the unequal elevation or depression of the object-end or eye-end of the telescope, in consequence of the unequal strength of the materials. If we suppose error to exist in each of the catalogues, this cause must have had an opposite influence in the two cases: if we compare the Greenwich observations with those of Dr. Brinkley, we shall arrive at the same conclusion; namely, that the differences must be caused by flexure in one or both of the instruments; since here also we find that the stars in the neighbourhood of the zenith are affected by only half the difference in polar distance, that is observed in the stars near the equator; and the same conclusions may be drawn from comparing the Greenwich observations with those of Mr. Bessel. The polar distances of all the stars in Mr. Bessel's catalogue exceed the polar distances given in the Greenwich catalogue; while those of all the stars in Dr. Brinkley's catalogue as regularly fall short of my determinations. It is not from the casual circumstance of my results being nearly a mean between the results of those two astronomers, that I intend to claim a superior weight of authority for my own; for, were this the only ground for preference, I should regard the question as yet undetermined, and should think it my duty to recommend the providing of new and more powerful instruments for ascertaining the truth. But it appears to me that from the observations by reflection, which I have lately made, and from their agreement with my observations by direct vision, that I am entitled to determine the share of error to which each of these two catalogues is liable; not only from the general superiority of the Greenwich circle, which I consider to have been thus proved, but from this peculiar circumstance, that whereas in the two catalogues of Mr. Bessel and Dr. Brinkley, the errors cannot fail to be the greatest in stars near the horizon; by my method of reflection, those stars which are nearest the horizon must be determined the most correctly, from their double altitudes being measured on the smallest arc.

In stars near the equator the catalogue of Mr. Bessel differs from that of Dr. Brinkley five seconds; and from the preceding considerations, I think we may venture to conclude that Mr. Bessel's polar distances are too great by about three seconds, and Dr. Brinkley's too small by about two: and since my catalogue differs from the two former from the zenith to equator in very nearly the same proportion, there can be no reason to doubt that their errors throughout are divided in nearly the same ratio.

With regard to the catalogue for the present period, which accompanies this paper, I beg to state that I consider it only

as

as a very near approximation to the truth, and requiring at least another year's observations, to render it of equal value with that of 1813, which is the result of two years observations with six microscopes, and in four positions of the telescope.

I am persuaded that the more this subject is considered, the more distinctly it will appear, that if any doubt can be entertained, founded on any circumstance arising out of the Dublin observations, that doubt must relate, not to the accuracy of former catalogues, but to the present position of the stars; since it is with respect to their *present* position that the two instruments are really at variance. This circumstance is very fortunate, as time may confirm the present or suggest some more satisfactory method of investigation, if what I have now advanced be not thought sufficient for the purpose.

XXXVII. *Chemical Researches by Dr.* Friedemann Göbel, *of Jena* *.

A. *Analysis of yellow Lead Ore.*

KLAPROTH has already given us, in his valuable "Contributions," an examination of this metallic salt, from which mine considerably differs both in the proportions of the component parts and in the means by which I determined them.

I obtained for analysis, through the kindness of M. Lenz, some very beautiful regular crystals of this substance. They were rectangular four-sided prisms, the lateral planes of which were uneven, dull, rough, and covered with a little carbonate of lime of a yellowish-white colour. The terminal planes, on the contrary, were smooth and shining, with a resinous lustre. The fracture was compact and obscurely lamellar. The colour of a wax-yellow. It was found at Bleiberg in Carinthia.

The crystals which were to be decomposed were first washed in dilute nitric acid, to separate the carbonate of lime adhering to them; then carefully washed in water, and dried.

I.—100 grs. reduced to a fine powder, and placed with sulphuric acid in the vacuum of an air-pump for 24 hours, only lost 0·02 grs.; their loss in water was equally small.

II.—100 grs. were dissolved by heat in dilute muriatic acid. When cool, a number of crystalline particles of chloride of lead were precipitated, and the precipitation was completed by a gentle evaporation of the liquid. The precipitate, collected on a filter, dried and ignited, weighed 72·5 grs. Now

* From Schweigger and Meinecke's *Nues Journal für Chemie und Physik; Neue Reihe, Band* 7, p. 71.

plumbane is a compound of 100 lead + 35 chlorine; therefore the above 72·5 grs. of plumbane contain 54·9 of lead, which combined with oxygen makes 59·0 oxide of lead.

III.—The fluid separated from the chloride of lead was now evaporated to dryness; and nitric acid was poured over the residuum, which produced by its decomposition a strong effervescence and evolution of nitrous gas; and the blue molybdous acid became again a yellowish-white powder (molybdic acid), which, when dried by evaporation and ignited in a coated crucible, weighed 40·5 grs.

According to this analysis, 100 grs. of the yellow lead ore consist of

Oxide of lead ...	59·0
Molybdic acid...	40·5
Loss...............	00·5
	100·0

The component parts, reckoned according to their proportions, correspond nearly to 1 proportion of molybdate of lead; and the regularly crystallized yellow ore is to be looked upon as such. It therefore consists of

One proportion of oxide of lead	= 107·5
One proportion of molybdic acid	= 77·5
	185·0

Taking a centesimal division, we have

Oxide of lead	58·1
Molybdic acid	41·8
	99·9

According to Klaproth, it consists of

Oxide of lead	64·42
Molybdic acid	34·25*
	98·67

B. *Tartarus Stibiatus.*

Some very fine and large regular crystals of this salt induced me to examine them; the result I obtained is given below. The crystals were about one inch long, and half an inch in diameter, and were very fine and transparent double four-sided pyramids.

* Although the results of Dr. Göbel's analysis of molybdate of lead differ so materially from those obtained by Klaproth, yet they nearly agree with Mr. Hatchett's, which were as follows:

Oxide of lead...........	58·40
Molybdic acid............	38·00
Oxide of iron............	2·08
Silica	0·28
Loss	1·24
	100·00

See Phil. Trans. for 1796, p. 323.—Edit.

I found

I found that 100 parts were composed of

Protoxide of antimony......	42·6
Tartaric acid	45·0
Potassa	9·8
Water...........................	5·75
	101·15

If we reckon these parts according to the laws of atomic combination, we find pretty nearly that emetic tartar may be considered as a compound of one proportion of sub-tartrate of protoxide of antimony, with half a proportion of neutral tartrate of potassa; and that one atomic proportion of it must be expressed by the number 231·7. For

Two proportions of protoxide of antimony $=2\times48=$	96·0	One prop. of protartrate of antimony.
One proportion of tartaric acid..........................	69·8	
Half a proportion of potassa.............................	22·5	Half a prop. of neutral tartrate of potassa.
Half a proportion of tartaric acid......................	34·9	
One proportion of water	8·5	
	231·7	

100 parts of this compound then consist of

Protoxide of antimony......	41·4
Potassa	9·7
Tartaric acid	45·1
Water...........................	3·6
	99·8

which agrees very nearly with the experimental result.

C. *A new Pyrophorus.*

While I was determining the proportions of the component parts of the tartrate of lead, I found that when it was heated in a glass tube, it produced a most beautiful pyrophorus.

When a portion of the dark-brown mass is shaken out of the tube, it catches fire immediately, and there appear on the surface of the ignited body, brilliant globules of lead, some of which become gradually changed into the yellow oxide, affording a most interesting spectacle.

The brilliancy continues much longer than in other pyrophori, so that, on account of its easy preparation, this might afford a convenient method of producing fire.

The inflammation of such pyrophoric substances has of late been attributed principally to potassium; but this pyrophorus gives us a new proof that other metallic compounds (as in this case the carburet of lead?) are susceptible of spontaneous inflammation on coming into contact with the air.

XXXVIII. *True apparent Right Ascension of Dr.* Maskelyne's 36 *Stars for every Day in the Year* 1823, *at the Time of passing the Meridian of Greenwich.* [Continued from page 111.]

1823.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 53	H. M. 4 25	H. M. 5 3	H. M. 5 6	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 30	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 40	H. M. 11 41	H. M. 13 15	H. M. 14 7
Oct.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	12·49	17·72	6.60	50·68	42·93	5·57	11·02	39·10	23·47	21·02	4·93	31·85	55·41	58·56	3·54	30·67	54·82	37·25
2	49	73	62	71	97	60	05	13	50	05	96	88	44	58	55	68	82	25
3	49	75	64	74	43·01	63	08	15	53	09	99	92	46	60	56	69	83	25
4	50	77	66	77	05	66	12	18	56	12	5·02	95	49	62	57	70	83	24
5	50	78	67	80	09	68	15	21	59	16	05	98	51	64	58	71	83	24
6	50	80	69	82	13	71	18	24	62	19	08	32·02	53	67	59	72	83	23
7	50	81	71	85	17	74	21	27	65	23	11	05	56	69	61	74	84	23
8	50	83	73	88	21	77	24	30	68	26	14	08	58	71	62	75	84	23
9	50	84	75	90	25	79	27	33	71	30	17	12	61	74	64	77	85	23
10	50	85	76	93	29	82	31	36	74	33	20	15	63	76	65	78	86	23
11	51	87	78	95	33	85	34	39	77	37	23	19	66	79	67	80	86	23
12	51	88	80	98	36	87	37	42	80	40	26	22	69	81	68	82	87	22
13	51	89	81	51·00	40	90	40	44	83	44	29	25	71	84	70	83	88	22
14	51	90	83	03	44	92	43	47	86	47	32	29	74	86	72	85	* 88	22
15	51	92	85	05	48	95	47	50	89	51	35	32	76	89	73	86	89	22
16	51	93	86	08	51	98	50	53	92	54	38	36	79	91	75	88	90	22
17	51	94	88	10	55	6·00	53	56	95	58	41	39	82	94	77	90	91	22
18	51	95	90	13	59	03	56	59	98	62	44	42	85	97	79	92	92	22
19	51	96	91	15	62	05	59	62	24·01	65	47	46	88	99	81	93	93	22
20	50	97	93	17	66	07	62	65	04	69	50	49	90	59·02	83	95	94	23
21	50	98	94	20	70	10	65	67	07	72	53	53	93	04	85	97	95	23
22	50	99	95	22	73	12	68	70	10	76	56	56	96	07	87	99	96	23
23	50	18·00	97	25	77	15	71	73	13	80	59	59	99	10	89	31·01	97	24
24	49	01	98	27	80	17	74	76	16	83	62	63	56·02	12	91	03	98	24
25	49	02	99	29	84	19	76	79	18	87	65	66	05	15	93	05	99	25
26	49	03	7·01	32	88	22	79	81	21	90	68	70	08	18	95	07	55·00	25
27	48	04	02	34	91	24	82	84	24	94	71	73	11	21	97	09	02	* 26
28	48	05	03	36	94	26	85	87	27	98	74	77	14	24	99	11	03	27
29	47	06	05	39	97	29	88	89	30	22·01	77	80	17	27	4·01	14	05	27

1823.	Libræ.	Libræ.	Bor.	pentis.	tares.	culis.	cii.	Lyræ.	Aquilæ.	læ.	læ.	Capri.	Capri.	Cygni.	Aqua.	malhaut.	gasi.	medæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 56	H. M. 23 59
Oct.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	57·21	8·63	13·60	35·67	37·34	37·29	45·85	58·91	54·05	12·24	40·67	53·92	17·75	26·91	45·68	56·08	1·36	19·96
2	21	63	59	66	33	27	83	88	04	22	65	91	74	89	67	07	35	97
3	20	62	57	65	32	26	82	86	02	21	64	89	72	87	67	07	35	98
4	20	62	56	64	31	24	80	83	00	19	62	88	71	84	66	06	34	98
5	20	62	55	63	29	22	78	81	53·98	17	60	86	69	82	65	05	34	98
6	19	61	53	62	28	21	77	78	97	16	59	85	68	80	64	05	34	98
7	19	61	52	61	27	19	75	76	95	14	57	83	66	77	63	04	33	98
8	19	61	51	60	26	17	73	73	93	12	55	81	64	75	62	03	33	98
9	18	60	50	59	25	16	72	71	92	11	54	80	63	72	61	02	32	98
10	18	60	49	58	23	14	70	68	90	09	52	78	61	70	60	02	31	98
11	18	60	48	58	22	13	68	66	88	07	50	76	59	67	59	01	31	98
12	18	60	47	57	21	11	67	63	86	06	49	75	58	65	58	00	30	98
13	17	59	46	56	20	10	65	61	85	04	47	73	56	62	57	55·99	30	98
14	17	59	45	56	19	08	64	58	83	03	45	72	55	60	56	98	29	98
15	17	59	44	55	18	07	62	56	81	01	43	70	53	57	55	97	28	98
16	17	59	44	54	17	05	61	53	80	00	42	69	52	55	54	96	28	98
17	17	59	43	54	16	04	59	51	78	11·98	40	67	50	52	53	95	27	98
18	17	59	42	53	15	03	57	48	76	96	38	66	49	49	52	94	26	98
19	17	59	41	53	14	01	56	46	75	95	37	64	47	47	50	93	26	98
20	17	59	41	52	13	00	55	44	73	93	35	63	46	44	49	92	25	97
21	17	59	40	52	13	36·99	53	41	71	92	33	61	44	42	48	90	24	97
22	17	59	40	52	12	98	52	39	70	90	32	60	43	39	47	89	23	97
23	17	59	39	51	11	97	51	37	68	88	30	58	41	36	45	88	23	97
24	18	60	38	51	11	96	50	35	67	87	29	57	40	34	44	87	22	96
25	18	60	38	51	10	95	48	32	65	85	27	55	38	31	43	86	21	96
26	19	61	37	50	09	94	47	30	64	84	26	54	37	29	41	84	20	96
27	19	61	37	50	09	93	46	28	62	82	24	52	35	26	40	83	19	95
28	20	62	37	50	09	92	45	26	61	81	23	50	33	24	39	81	18	95
29	20	62	37	50	09	91	44	24	59	79	21	49	32	21	37	80	17	94
30	21	63	37	49	09	90	43	22	58	78	20	48	31	19	36	79	16	94
31	22	64	37	49	08	89	42	20	56	76	18	46	29	16	35	78	15	93

N.B. On those days where an Asterisk is prefixed the Star passes twice; the Æ given is that at the first passage.

XXXIX. *Remarks on the Identity of certain General Laws which have been lately observed to regulate the Natural Distribution of Insects and Fungi.* *By* W. S. MacLeay, *Esq. M.A. F.L.S.**

THE naturalists of the present day have in one respect a peculiar claim to the appellation of disciples of Linnæus; inasmuch as they direct their chief attention to what this great master declared to be the end of all his immortal labours in botany. His admirable maxim, that the natural system is the "*ultimus botanices finis*," is now not only universally admitted, but on all sides acted upon. The natural system is in fact not only made the remote consequence, but the immediate aim, of every modern observation in natural history; the rule now being to commence with supposing nothing known but what has actually been observed, and by comparing the affinities thus collected, to search after that knowledge of natural groups which in the old methods we started with supposing to be already acquired. They who formerly confined themselves to artificial systems, and neglected the above important maxim of Linnæus, have at least thereby lost much gratification, since, if there be nothing within the whole range of human science more worthy of profound meditation than the plan by which the Deity regulated the creation; so most assuredly no study is more calculated to administer pure and unmixed delight. Thus, for example, the satisfaction of the mere gazer at a collection of animals must evidently be inferior to that experienced by the comparative anatomist, who understands their respective structures. And again, the anatomist himself, on viewing a museum, can scarcely be so much gratified by the sight, as that naturalist who, not content with a bare and in some degree insulated knowledge of particular organizations, endeavours to comprehend how these harmonize with the rest of the creation. It is in this last mode alone, if I may so express myself, that the human mind can take, as far as its imperfect nature will permit, a view of the universe as it was originally designed. Nor ought any person to be deterred from commencing so delightful a pursuit, either by the supposed difficulty of the investigation, or by the extent of preparatory information which it necessarily requires: for truly has it been said, that he who questions his abilities to arrange the dissimilar parts of an extensive plan, or fears to be lost in a complicated system, may yet hope to adjust a few pages without perplexity.

* From the Transactions of the Linnean Society, vol. xiv. part I.

Having

Having such ideas both of the dignity of natural history and of the importance and feasibility of a more extended research into the natural system than has yet been made, we can scarcely fail to be interested by a late work*, of which the perusal has induced me to address this learned body. Although this work is confined to a department of botany not very generally studied, its author has evidently not been satisfied with the specific discrimination of the imperfectly organized subjects of his research, but has earnestly sought to discover the relations which they bear to each other. Keeping this object steadily in view, M. Fries has been able to give so connected and symmetrical an outline of what he considers to be the natural distribution of *fungi*, as, at least in my opinion, to merit the careful attention of zoologists as well as botanists. It will readily be imagined that, in saying this much, I do not, in the presence of so many more able judges, presume to advance any positive opinion on his merits as an observer. I confine myself entirely to that theory or reasoning founded by M. Fries upon the general result of observations, which it would be impossible to suppose altogether incorrect, even if his reputation as a cryptogamist were less than it really is. On this head, however, I have to remark that our author, although undoubtedly an original observer, is neither the first who has advanced this theory, nor do *fungi* compose the only part of organized matter in which this sort of arrangement has been conceived to exist. So that even with respect to his theory I may be a partial judge, and may probably be more inclined to admit the validity of his conclusions, than will be deemed prudent by others who are altogether unprejudiced.

M. Fries justly remarks, that the notion of the celebrated Bonnet, as to the existence of a simple series or chain of natural affinities, has been long exploded. The truth however is, that the law of continuity has been quite misunderstood both by Bonnet, and his opponents, so far as organized matter is concerned: for Bonnet fancied that, if affinities were continuous, the series must therefore be simple: and some modern naturalists finding by experience the series not to be simple, therefore supposed that affinities could not be continuous, but that nature presents to the view a mass of unconnected groups, in which it would be a waste of time and a loss of labour to search for any general plan. It does not however appear that either of these inferences has been very philosophically drawn; for there is a certain rule in natural history which originates

* *Systema Mycologicum* sistens Fungorum Ordines, Genera, Species, &c. quos ad Normam Methodi Naturalis determinavit, disposuit atque descripsit Elias Fries, &c. vol. i. Gryphiswaldiæ, 1821.

solely in observation, and which, if properly followed up, will infallibly induce us to grant to Bonnet the truth of his proposition, that affinities are continuous, and yet to agree with his opponents that the series of natural beings is not simple. This rule is, that *Relations of Analogy must be carefully distinguished from Relations of Affinity*; for, as our author M. Fries most truly says, "*Quo magis in superficie acquieverunt naturæ scrutatores, eo magis analoga cum affinibus commutârunt.*"

The ideas of Affinity and Analogy are so distinct from each other in the mind of every person acquainted with the first principles of logic, that even while this distinction was not laid down as an axiom in natural history, experienced naturalists perceived that every correspondence of character did not necessarily constitute an affinity. Thus the celebrated Pallas, in his *Elenchus Zoophytorum*, has well observed that Bonnet, in order to complete his linear scale of nature, was obliged to abandon the true vinculum of affinity, and to resort to such superficial or analogous characters as those which connect *Vespertilio* and *Exocœtus* with birds. But the nature of the difference which exists in natural history between affinity and analogy, was I believe first discovered in studying Lamellicorn insects; and in the year 1819, when I published that discovery*, the fifth part of an acute philosophical work, entitled Botanical Aphorisms†, appeared in Sweden, wherein the distinguished cryptogamist M. Agardh proves by the following words, that he likewise had a slight glimpse of the same truth: "Analogia quædam et similitudo in diversis seriebus vegetabilium interdum cernatur, quasi progressa esset natura ad perfectionem per eosdem gradus sed diversâ viâ‡."

* The 1st Part of *Horæ Entomologicæ* is here alluded to.—Edit.

† *Aphorismi Botanici*, quos veniâ Ampliss. Ord. Philos. Lund. Præside Carolo Ad. Agardh, &c. pro Gradu Philosophico, p.p. N. Kuhlgren, &c. p. v. Lundæ, 1819.

‡ In the same little tract M. Agardh makes two other observations, which coincide with what I have noticed in the animal kingdom. The first is as follows: "Inter inferiores formas superiores sæpe efflorescunt, sed rudes et veluti experimenta: sic anticipationes formæ perfectioris in plantis inferioribus non raro obveniant; ut etiam in plantis superioribus regressus ad formam imperfectiorem." Now in the *Horæ Entomologicæ*, p. 223, I have attempted to show that Nature, in the imperfectly constructed *Acrita*, sketches out in a manner the five principal forms of the animal kingdom. So also the direct return of Annulose *Vermes* to *Acrita* is repeatedly asserted in the same work: this however seems to depend more properly on M. Agardh's other observation, viz. "Duplex est itaque affinitas plantarum, aut ea, quæ oritur e transitu ab unâ formâ normali ad alteram, aut ea, quæ versatur imprimis in anticipatione formæ superioris aut regressu in formam inferiorem. Illam affinitatem *transitus* appellamus, hanc *transultationis*." This affinity of *transultation* is evidently nothing else than the disposition observable in opposite points of the same series or *transitus* of affinity to meet each other, and of which I have given various examples in the *Horæ Entomologicæ*, p. 319.

The

The next work in which the distinction appeared was the *Mémoires du Muséum d'Histoire Naturelle;* in a part of which, published in the autumn of 1821, a paper was inserted by M. Decandolle on the natural family of *Cruciferæ*. Here this botanist states, that he finds it possible to express in a table all the affinities existing in this family of plants by what he terms a *double entrée;* in other words, he supposes that there are transversal affinities as well as direct ones,—a notion of the reality however which appears to be much more confused than that previously entertained by M. Agardh and explained as above in his Botanical Aphorisms.

In the same year (1821) likewise appeared the abovementioned work of M. Fries on *Fungi*, which is explicit on the subject, and wherein the very same expressions of affinity and analogy are used to designate these different relations, which I had applied to them two years before in treating of Lamellicorn Insects *.

The theoretical difference between Affinity and Analogy may be thus explained †: Suppose the existence of two parallel series of animals, the corresponding points of which agree in some one or two remarkable particulars of structure. Suppose also, that the general conformation of the animals in each series passes so gradually from one species to the other, as to render any interruption of this transition almost imperceptible. We shall thus have two very different relations, which must have required an infinite degree of design before they could have been made exactly to harmonize with each other. When, therefore, two such parallel series can be shown in nature to have each their general change of form gradual, or, in other words, their relations of affinity uninterrupted by any thing known; when moreover the corresponding points in these two series agree in some one or two remarkable circumstances, there is every probability of our arrangement being correct. It is quite inconceivable that the utmost human ingenuity could make these two kinds of relation to tally with each other, had

* I owe my acquaintance with these several works, as well as much information on points of which I should otherwise have been totally ignorant, to the friendship of the consummate botanist, in whose possession the Banksian Library has been so worthily deposited. The second part of the *Horæ Entomologicæ* was published in April 1821. On the 24th of the following month I first saw a copy of M. Decandolle's paper, which was not published till some weeks after, and in the course of last winter I first saw Agardh's paper and the work of M. Fries on Fungi. If M. Fries borrowed from his master Agardh the idea of distinguishing affinity and analogy, which is not improbable, we must at least allow him the merit of having greatly improved this part of the theory.

† See *Horæ Entomologicæ*, p. 362 *et seq.*

 they

they not been so designed at the creation. A relation of analogy consists in a correspondence between certain parts of the organization of two animals which differ in their general structure. In short, the test of such a relation is barely an evident similarity in some remarkable points of formation, which at first sight give a character to the animals and distinguish them from others connected with them by affinity; whereas, the test of a relation of affinity is its forming part of a transition continued from one structure to another by nearly equal intervals. As a relation of analogy must always depend on some marked property or peculiarity of structure, and as that of affinity, which connects two groups, becomes weaker and less visible as these groups are more general, it is not in the least surprising, that what is only an analogical correspondence in one or two important particulars, should often have been mistaken for a general affinity.

M. Fries draws the distinction between them precisely in the same way, and, making allowance for the difference of the objects he was investigating, almost in the same words: "Natura tamen, ubique varia, semper tamen eadem, hoc est, eandem ideam exponere tendit, mutatis modo, quæ ex ulteriori ratione necessario pendent; eadem sequitur principia, ita modo ut inferiora (v. g. exterior forma, quæ in infimis adhuc vaga) superioribus cedant. Errant igitur qui distinctiones summas e formâ exteriori tantum ducunt; quis ex hâc regnum animale et vegetabile definire potuit? Evidentissimè hoc demonstrant Lichenes et Fungi. Recentiores, horum differentiam in characteribus externis tantum ponentes, cum Fungis jungere voluerunt *Leprarias*, *Opegraphas*, *Calicia*, *Verrucarias*, &c. quod nullo modo probare possum. Altius illorum differentia deducenda. Sed cum natura eâdem viâ inter Lichenes et Fungos ubique progreditur, singulum genus Lichenum Fungis correspondet. At hæc inde *affinia* non dicimus; sed *analoga*.

"*Affinia* igitur sunt quæ in eadem serie sequuntur et in se invicem transire videntur. Hæc in ulterioribus congruunt sed in citerioribus rationibus differunt. *Analoga* autem dicimus quæ in diversis seriebus locis parallelis* posita sunt et sibi invicem correspondent. Ultima cosmica momenta differunt,

* As there is some danger of being led astray by our imagination when we first attempt to separate relations of analogy from those of affinity, it is fortunate that the naturalist cannot have a more admirable test of his accuracy, or a stronger rein on his fancy, than this parallelism of analogous groups in contiguous series of affinity. Thus, although a solitary resemblance may mislead, it is clear that when we find several of such resemblances to keep parallel to each other in contiguous series, we may reckon upon their having some more solid foundation than our own fancy.

ferunt, sed citeriora congruunt, quæ in habitu externo et characteribus accidentalibus mutandis maxime valent. Ubicumque in Historiâ naturali oculos convertimus, singulum organismum multiplicia hujus offerunt exempla. Systema mycologicum infra explicatum his omnino nititur. *Clavaria* et *Peziza*, *Biatora* et *Bæomyces* affines sunt; sed *Clavaria* et *Bæomyces*, *Peziza* et *Biatora* analogæ, *e. s. p.* in infinitum.

"Comparatio Linnæana affinitatis plantarum cum mappâ geographicâ haud ignobilis visa fuit; ignoscatur igitur mihi hanc ita extendenti, ut affinitas in hâc indicet longitudinem et analogia latitudinem.

"Neque hoc tantum in inferiores classes quadrat. Naturæ leges ubique harmonicæ. Si systema mycologicum et principia quibus nititur, omnibus non displicerent, totius regni vegetabilis dispositionem demonstrare conabor. Plurima jam elaboravi."

Relations of affinity being thus separated from those of analogy, we immediately get the following facts from the observation of what M. Agardh terms the affinity of *Transitus*, namely, that species form the only absolute division in nature, and that no groups of species (whatever may be the rank of these groups) ought to be considered as insulated, but only as series of affinities returning into themselves, and forming as it were circles which touch other circles. Such only are natural groups. This was said of insects*; and our author, looking only at plants, and principally at *Fungi*, comes to the same conclusion, as appears from the following words: "Species unica in naturâ fixè circumscripta idea. Superiores nullas agnovimus sectiones strictissimè circumscriptas, tantum circulos plus minus clausos, affines vero ubique tangentes. Hos tribus, genera, sectiones, &c. simulque si naturæ vestigia sequuntur, naturales dicimus."

That the circle, indeed, is not always closed or complete has been observed likewise in the animal kingdom; and there are two ways of accounting for it. First, that the beings which would render the circle complete have not yet been discovered; a conclusion to which we readily arrive on considering how little is yet known of natural productions; and secondly, that there are *hiatus* or chasms which do really exist in nature, and which may be attributed to the extinction of species in consequence of revolutions undergone by the surface of this globe. Whether one only or both of these reasons be requisite to account for circles of affinity not always appearing complete, we shall not at present investigate; contenting ourselves

* *Horæ Entomologicæ*, p. 459, &c.

with

with the undoubted fact, that *hiatus* or chasms are everywhere in nature presenting themselves to the view. But this truth by no means contradicts the Linnean maxim, that no *saltus* exists in nature, although such has been esteemed its effect by certain naturalists who have been in the habit of taking the words *hiatus* and *saltus* as synonymous terms *. Thus the series of the *Systema Naturæ* and of the *Règne Animal* is not natural where the *Cetacea* intervene between Quadrupeds and Birds, but is perfectly consonant with nature where the Tortoises are made to follow these last. In the first case, there is a *saltus* or leap from Quadrupeds to Birds over a group totally dissimilar to the latter; there is, in short, an unnatural interruption of the law of continuity, which shocks not merely the naturalist but the ordinary observer. In the other case there is only an *hiatus* or chasm, which the discoveries of a future day may fully occupy. Speaking therefore theoretically, it may be affirmed that a *saltus* never did exist in nature; and it also may be argued, with great appearance of truth, that if the *hiatus* are real which so commonly occur in nature, they did not always exist; or, in short, as M. Fries expresses himself, "*Omnis sectio naturalis circulum per se clausum exhibet.*"

Now this definition of a natural group could never have been given by any person who was not aware of the distinction to be made between affinity and analogy. But whenever two parallel series of objects linked by affinity are drawn up in array, the connexion of their extremes, that is, the formation of the circle, becomes in that very moment, so far as I have observed, more or less conspicuous.

It follows, moreover, from admitting the existence of analogical relations, or, in other words, from laying down the parallelism of groups in different series of affinity, that the number of groups in these series must be the same. For were it otherwise,—as for instance, supposing three groups to exist in one complete series, and four in another,—it is clear that the parallelism could not exist. But if this parallelism be real, which has been, as shown above, asserted independently of each other by several naturalists acting in different branches of natural history, then the number of groups of the next lower order composing a group of a given degree must be determinate. And if, moreover, we accord to our author the accuracy of the following rule, namely, "Nunquam negligendum, unumquodque

* It is to be regretted that Professor Dugald Stewart should have been led into this common error, and thus have acquired a somewhat erroneous notion of the law of continuity as it refers to natural history. See the second part of his admirable Dissertation, as prefixed to vol. v. of the *Supplement* to the *Encyclopædia Britannica*.

regnum

regnum, ordinem, genus, &c. in systemate ut individuum esse sumendum;"—in other words, that class bears the same relation to class which order does to order, and genus to genus; then the number of groups composing *any* group of the next higher degree must be determinate; and it only remains for the naturalist to discover from observation what this number is.

That Nature has made use of determinate numbers in the construction of vegetables has long been known empirically; as for instance, where botanists have found the typical number of parts of fructification in the acotyledonous plants of Jussieu to be two, that in monocotyledonous plants to be three, and that in dicotyledonous plants to be five, or multiples of these numbers. Consequently the existence of a determinate number in the distribution of the plants themselves might have been argued *à priori*. And in this manner indeed M. Fries appears to have argued; for it is tolerably clear that it was the consideration of the foregoing rule, adopted by Nature in the structure of acotyledonous plants, which induced him theoretically to assume four as a multiple of two to be the determinate number in which *Fungi* are grouped *. I say this, because he is obliged from actual observation to admit that of these four groups, one is excessively capacious in comparison with the other three, and is always to be divided into *two*. So that we may either, with M. Fries, consider every group of *Fungi* as divisible into four, of which the largest is to be reckoned as two,—a supposition that would not only make two determinate numbers, but which, from the binary groups not being always analogous, will moreover break the parallelism of corresponding groups,—or we may account every group as divisible into five, and thus not only agree with M. Fries's observations, but besides keep the parallelism of analogies uninterrupted. If in this state of the matter it could now be shown, that in the animal kingdom the same law is followed by nature; in short, to take an instance, if it could be proved that the *Annulosa* may either be divided into four groups, viz. *Ametabola*, *Crustacea*, *Arachnida* and *Ptilota*, where this last is remarkably capacious and divisible into two natural groups, viz. *Mandibulata* and *Haustellata*, or that annulose animals may be divided at once into five groups of the same degree, but of which two have a greater affinity to each other than

* It ought here to be observed, that Ocken had previously advanced the opinion that four was the determinate number in natural distribution. This naturalist, however, having in his *Natúrgeschichte für schulen*, lately published, in a great measure abandoned the number four for five, and that more especially in the animal kingdom, has thus got into all the difficulties which necessarily attend the supposition of two determinate numbers.

they

they have to the other three—if, I repeat, this could be proved, should we not be justified in affirming that the rule, so far as concerns Insects and Fungi, is one and the same? The possibility of thus distributing the annulose animals has, however, been demonstrated already in the *Horæ Entomologicæ;* and it is the way in which we ought to take the rule that only now remains to be investigated. In short, since only two methods* have yet been found to coincide with facts as presented by nature, the question is, whether we ought to account *Fungi* as divisible into five groups,—or into four, of which one forms two of equal degree. Now I think it may without difficulty be shown, from our author's own observations and rules, that there is only one determinate number which regulates the distribution of *Fungi*, and that five is this number.

[To be continued.]

XL. *A few Observations on the Natural Distribution of animated Nature. By* A Fellow of the Linnean Society.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, May 1823.

As the natural arrangement of animated forms has gradually advanced into a very dignified and important science, a few remarks on the subject, although anonymous, may not be unacceptable to your zoological readers: because the objects themselves are so immediately or remotely connected with every thing we esteem, and are withal so multifarious, that much more remains for future ages to develop, than has already been achieved.

* The number seven might also perhaps, for obvious reasons, occur to the mind, were it allowable in natural history to ground any reasoning except upon facts of organization. The idea of this number is however immediately laid aside, on endeavouring to discover seven primary divisions of equal degree in the animal kingdom. It is easy, indeed, to imagine the prevalence of a number; the difficulty is to prove it. The naturalist, therefore, requires something more than the statement of a number, before he allows either a preconceived opinion or any analogy not founded on organic structure to have an influence on his favourite science. He requires its application to nature and its illustration by facts. As yet, however, no numbers have been shown to prevail in natural groups but five,—or, which is the same thing, four, of which one group is divisible into two. Perhaps, indeed, the most clear method of expressing ourselves on this subject is to say that, laying aside osculant groups, every natural group is divisible into five, which always admit of a binary distribution, that is, into two and three.

Assuming

Assuming the existence of *two* primæval principles, one sentient—the other not, viz.

MIND......and......MATTER,

the place of whose existence is *space*, and the period of it *time*; whose continuity constitutes *eternity*; the writer ventures to conclude, that a Binary Distribution of natural objects was at least the primæval one *, however altered or modified by causes and effects of subsequent occurrence, and themselves amenable to ulterior variations. And it is very remarkable that most, or all, of Nature's *superior divisions*, *are* actually, or virtually, either duplications, or multiples, of the numeral Two.

Nor is it until we arrive amongst the groups which constitute as it were the interiors of the vegetable and animal kingdoms, in ascending the great scale of creation, that 5 appears clearly a very frequent, if not an universal number, circularly disposable, and as it were returnable into itself, according to the elaborate theory of MacLeay in his learned *Horæ Entomologicæ* recently published.

Of the two supernal divisions, the first, that is, MIND or SPIRIT, is an *unit* and absolutely indivisible, although creation is replete with it; for it actually occupies, in endless variation, every animated form; "ubique varians, semper tamen eadem." But the other supernal division, MATTER, separates into no less than *three* modifications, viz.

1. *Unorganized*,
2. *Crystallized*,
3. *Organized*.

And the latter divides into

Animal and *Vegetable*.

And all these, if thus placed,

Unorganized,
Organized,
Crystallized,

would form what may be called the *first* circle of Nature returning into itself, and from whence emanate, in endless order, all things that exist:

"MENS agitat molem, magnoque se corpore miscet."

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* It has occurred to us that the poet Ausonius (Eidyll. xi.) hints at a *ternary* arrangement, commencing in a manner somewhat similar:

"In Physicis *tria* prima, DEUS, MUNDUS, DATA FORMA.
Tergenus omnigenum, genitor, genitrix, generatum."

But some, perhaps, will think that those who begin so high, and classify abstractions and qualities, should say, not "*in physicis*," but "*in metaphysicis*."——EDIT.

 Yet,

Yet, notwithstanding the apparent probability of this *ternary* arrangement, the writer inclines in favour of the *binary* distribution, together with its duplications, as insisted on by Fries (in his celebrated work on Fungi) and others; for in that manner much, in verity, may be advanced.

Thus, supposing that of the two primæval principles MIND (*i.e.* SPIRIT) and MATTER, the latter divided itself into two (*organized* and *unorganized*); and that these again each separated into two more, viz. the former into *vegetables* and *animals*, the latter into *crystallized* and *uncrystallized;* according to the plan subjoined to this paper, we should appear to proceed in a clear light, and without perhaps any objection.

The two latter of these secondary divisions (*crystallized* and *uncrystallized*) are, as it were, sterile, and proceed no further, at least into *primary* divisions; while from the *organized* exuberant *root* advance, in the most beautiful and harmonious order, all the multifarious branches of animated Nature:

> "Spiritus intus alit; totamque infusa per artus."

Of the vegetable and animal groups, each of which are so often and so repetitely divisible into *fives*, forming circles naturally returning into themselves, the writer has not at present leisure to consider, otherwise than the annexed plan itself may show; but he reserves for a more favourable opportunity the remaining details of this most interesting subject.

The Plan *alluded to above, dividing* Matter *in a binary manner.*

MIND——MATTER

Organized——Unorganized

crystallized—uncrystallized

Animal——Vegetable

Vertebrate—Invertebrate

Cotyledonous——Acotyledonous

Cryptogamous—Agamous

Dicotyledonous—Monocotyledonous

Apetalous—Petalous

Monopetalous—Polypetalous

Culmaceous—Liliaceous

Triandrous—Hexandrous

Bracteaceous—Spathaceous.

XLI. *On*

XLI. *On the Firing of Gunpowder by Fulminating Mercury.* By Mr. E. G. Wright.

To the Editors of the Philosophical Magazine and Journal.

IT has been a just subject of complaint with sportsmen who use the percussion lock to their guns, that the powder made with the chlorine of potass has a tendency to promote rapid oxidation in the barrel and lock; besides generating dirt from the charcoal, after firing. I have found this inconvenience myself, and was induced to seek a remedy by adopting a different substance, in which I succeeded to my satisfaction; and I shall feel obliged if you will allow me, through your means, to make known the discovery, which may not only prove interesting to sportsmen, but to many of your scientific readers also. In November last Mr. Murray delivered his scientific and instructive lectures on chemistry in this city; and in consequence of his observations on *fulminating mercury*, some experiments I had made with that substance several years ago in firing gunpowder, were recalled to my recollection; and soon after he left us I was induced to make the powder, and try it with the copper caps, when I found it in every respect superior to the chlorine of potass preparation, and shot with it the remainder of the winter. Its advantages are:—It does not create rust so rapidly as the powder now used; it is not affected by damp or moisture; and from every severe test I have given it, I do not believe it so liable to explode,—and in case of such accident, as its force does not extend so far, its effects would not be so destructive. I am aware it is asserted that fulminating mercury will not fire gunpowder; but if any one has a doubt on this point, by procuring a percussion gun he may try the experiment and be fully satisfied, taking care, in loading, that the gunpowder is forced by the wadding to the point of contact with the fulminating compound.

My method of preparing the fulminating mercury is as follows:—I place two drachms of quicksilver in a Florence flask, and pour six drachms (measure) of *pure* nitric acid on the mercury: this I place in a stand over a spirit-lamp, and make it boil, till the quicksilver is taken up by the acid;—when nearly cool, I pour it on an ounce (measure) of alcohol in another flask: sometimes *immediate* effervescence ensues, with the extrication of nitrous ether; and often I have been obliged to place the mixture over the lamp, till a white fume begins to rise, when the effervescence follows. I suffer the process to continue (removing the lamp) till the fumes assume a reddish hue: when I pour water into the flask, and the powder is found

precipitated to the bottom, I pour off and add fresh water, permitting the powder to subside each time before the water is poured off, so as to free the substance as much as possible from the acid; and then I pour it on a piece of filtering paper, and place the powder in an airy room to dry. It should be kept in a corked (not stopper) bottle. Sometimes the powder is quite white, and often light brown, in colour; but this is of no consequence. To fill the caps, I use a small ivory pin, scooped at one end to take up the powder, and flat at the other end to fit the bottom of the cap: I place a very small portion of the powder in the cap, just sufficient to cover the bottom, and then dip the flat end of the pin in a strong tincture of gum benzoin, so as only to moisten it, (if I may be allowed the expression,) and press the pin so moistened on the powder in the cap, and gently turn it, so as to secure the powder in the cap, the tincture acting as a varnish on the surface of the powder. After a little practice, a great number of caps may be prepared in a short time in this manner; and I have no doubt the fulminating mercury will be preferred, on trial, to the percussion powder at present used. Several of my sporting friends have tried some caps I gave them charged with the fulminating mercury, and all agree as to its superiority to the common preparation from chlorine of potass.

I am, gentlemen,

Your very obliged servant,

Hereford, Sept. 18, 1823. E. G. Wright.

P.S. The fulminating mercury ought to be made in an outhouse, or in an unfurnished room, under a chimney, on account of the nitrous fumes extricated in the first, and the nitrous ether in the second part of the process. It may be made into a paste with weak tincture of gum benzoin, and granulated, for the magazine locks of Forsyth and other makers, but must not be mixed with any other substance.

XLII. *Experiments on the Development of Electricity by Pressure;—Laws of this Development.* By M. Becquerel, *Ancien Chef de Bataillon du Genie* *.

Statement of the Phænomena.

Coulomb, in a series of researches respecting the development of electricity by friction, was led to conjecture that the dilatation and compression experienced by the particles of the surfaces of bodies had a determinative influence upon the nature of the electricity developed by each of them. M. Biot,

* From the *Annales de Chimie et de Phisique*, tom. xxii. p. 5.

Biot, in his *Traité de Physique*, cites from the manuscripts of that celebrated philosopher the observations upon which he was induced to found this conjecture.

An experiment made by M. Libes with gummed taffeta seems to accord with this view of the subject. This experiment consists in taking a disk of metal, which is held by an insulating handle, and pressing it on gummed taffeta; the taffeta acquires the vitreous electricity, and the disk the resinous electricity. The effect is the more striking in proportion as the pressure is stronger; but it ceases as soon as the taffeta has lost that glutinosity which renders its surface easily compressible. If, on the contrary, the metal is rubbed over the taffeta, the metal takes the vitreous, and the taffeta the resinous electricity. Having recently had occasion to repeat the excellent observations of M. Haüy on the electrical properties which simple pressure with the fingers imparts to Iceland spar and to some other mineral substances, I was struck with the different effects produced by the bodies between which they were pressed, accordingly as they were more or less flexible. I wished at first to examine, in these and the preceding experiments, what might be the proper influence of the condensation of parts on the development of electricity, not only in minerals, but in other bodies, susceptible, like them, of experiencing this effect. In the course of this examination I have been led to a general result, which seems to promise, one day, to throw light on the yet unknown causes of the development of electricity. This result may be expressed in the following terms: When any two bodies whatsoever, one of which is elastic, are insulated, and pressed one against the other, they remain in two different electric states; but the excess of contrary electricity which they retain, on escaping from the compression, is in proportion as one of the bodies is what is called a bad conductor. The effect thus produced in this latter case, is incomparably more powerful than those arising from simple contact in the experiments of Volta.

The most simple method of obtaining these results consists in forming small disks of the substances with which the experiment is to be tried, of the thickness of some *millimetres;* they are fitted to handles, by which they are perfectly insulated*. One of these handles is then taken in each hand, and the

* Each handle is composed of a solid glass tube (*tube plein en verre*) covered with lac varnish, and terminated by a wooden knob, which is used in order to avoid the friction of the hand upon the glass. The small disks are fixed at the extremity of the tubes with lac. Before using this instrument, it is advisable to try with the electroscope whether the handle exhibits

the substances are pressed, for an instant, one against the other. After withdrawing them from contact, the quantity of electricity acquired by them is ascertained by the electroscope. A single contact is usually sufficient to repel the small disk of the electroscope of Coulomb; but on repeating these contacts, any electroscope whatever may be strongly charged. Sometimes the electricity is so strong, that the disk immediately attracts the small light bodies which are presented to it. Let us suppose, for instance, two insulated disks, the one of cork, the other of caoutchouc; after pressure, the latter has acquired the resinous, and the former the vitreous electricity. If we press, in the same manner, the cork on the rind of an orange, both being insulated, the cork acquires the vitreous electricity, and the orange-peel the resinous. Finally, the orange-peel, pressed on the caoutchouc, takes the vitreous electricity, and imparts the resinous to the caoutchouc.

Pressure exerted upon insulated mineral substances produces analogous effects. Iceland spar, sulphate of lime, fluate of lime, sulphate of barytes, &c., when pressed by the disk of cork, acquire an excess of vitreous electricity, whilst the disk itself contracts an excess of resinous electricity. Disthène and retinasphaltum, on the contrary, have the resinous electricity.

Coal, amber, copper, zinc, silver, &c., when pressed by the insulated disk of cork, receive an excess of resinous electricity, and the cork receives an excess of the vitreous.

In all the preceding experiments, the two substances subjected to pressure were insulated, in order that the species of electricity acquired by each of them might be separately studied; but, as might be expected, the same effects take place when a single body is insulated, and the other communicates with the common reservoir. The insulated body then acquires by pressure the same kind of electricity as when the body upon which it was pressed was also insulated; but the electricity acquired by the latter cannot be perceived, since it escapes into the earth.

For instance, an insulated disk of cork, pressed upon Iceland spar, fluate of lime, sulphate of lime, &c., acquires the resinous electricity; but when pressed upon copper, zinc, and the other substances, it retains, after the compression, an ex-

exhibits any marks of electricity. If any appear, the electricity may be expelled by heating the tube in the flame of a taper. In order to ascertain to what degree the lac may influence the electric effects of disks by pressure, these disks must be pressed hard upon bodies incapable of giving out much electricity; it is then perceived whether or not a development of electricity takes place.

cess

cess of vitreous electricity. Even fruits, as, for instance, the orange, being slightly compressed by the disk of insulated cork, communicate to it an excess of vitreous electricity. In proportion as the fruit dries, its power of electrifying the cork diminishes. When ripeness has given it all the elasticity of which it is susceptible, and before its surface is moistened by decomposition, this power appears to be at its height.

The insulated cork, applied with pressure upon all parts of animals, provided they are not moist, receives an excess of resinous electricity. The hair and fur of animals communicate to it nearly as much as Iceland spar would do, but it is of a contrary nature.

Imperfect liquids, when sensibly compressible, give analogous results. Cork, slightly pressed upon oil of turpentine thickened by fire, exhibits, after the pressure, an excess of resinous electricity.

I have hitherto only considered the pressure of a disk of cork upon different substances; but similar results would be obtained by the pressure of disks of leather, of amadou, or of elder-pith, upon the same substances.

Bodies which have acquired electricity by pressure, preserve it for a longer or shorter time according to the degree of their conducting power. M. Haüy found that Iceland spar gave some signs of electricity, even at the end of eleven days. There are other bodies, which are such good conductors, that, when not insulated, they part with the excess of electricity they have acquired, to the substances with which they are in contact. The sulphate of barytes of Royat is of this number: it is necessary to insulate it perfectly, in order to preserve its electricity. A crystal, which had been subjected to the experiment, possessed the electric faculty at the end of half an hour. It is very probable that the continuance of electricity in bodies is in proportion to their conducting power. This preservation of electricity in certain bodies, notwithstanding the absorbing action of the air, and even notwithstanding the contact of the moist substances by which they are surrounded, has been satisfactorily proved by M. Haüy. May it not be accounted for by supposing that the electricity developed by pressure at the surface of these bodies acts on the natural electricity of their masses, decomposes it, attracts that of a contrary denomination, and drives the other into the centre of the mass, in such a manner as to transform these bodies into actual condensers, precisely as when an electrified plate is placed on the marble plate of Volta's condenser?

On

On the Causes which modify the Development of Electricity by Pressure.

In the account I have just given of the electrical phænomena produced by pressure, I have only pointed out the manner of repeating the experiments, without speaking of the causes which might possibly modify the results: but as these causes have more or less influence on the development of electricity, and may even sometimes render it null, it is necessary to examine them.

The more or less perfect conducting power of the two bodies subjected to pressure, has a singular influence on the quantity of electricity produced. If, for instance, a disk of elder-pith and one of metal are pressed together, neither of them, when the pressure is withdrawn, will be found to have acquired any excess of electricity; and this will be the case whenever the substances pressed are conductors; each of these substances will possess only the quantity of electricity due to the contact. In general, it appears that the more perfectly the two bodies possess the quality of conductors, the more difficult it becomes to obtain electricity by pressure.

We are ignorant of what passes during this action: nevertheless the electric phænomena we have observed, permit us to hazard some conjectures on this subject. It appears, that at the moment of pressure there is produced a new state of equilibrium between the two fluids which compose the natural electric fluid; the vitreous electricity takes possession of one of the surfaces in contact, and the resinous electricity of the other. As long as the pressure continues, these two fluids are neutralized by each other, and they cannot escape from the surface of contact. Thus, notwithstanding the reciprocal attraction of their molecules, notwithstanding their greater or less tendency to pass from one body to another, they find in pressure, and in pressure alone, a power which neutralizes both these actions. In fact, if the bodies be perfect conductors, as soon as a diminution of pressure takes place, the two fluids instantaneously combine, however great may be the rapidity of the separation: if, on the contrary, one of the two bodies be an imperfect conductor, a diminution of pressure is not immediately succeeded by the recomposition of the two fluids, the devevelopment of which arose from the cessation of the pressure. This recomposition will occupy more or less time in the ratio of the conducting power of the two bodies subjected to pressure; so that, in the end, the quantity of electricity found in each of the

the bodies, will be exactly that due to the remaining pressure. Let us take, for instance, two insulated bodies, such as a disk of cork and a crystal of sulphate of barytes, conveniently disposed; let us press them one against the other with the pressure p; let us diminish the pressure by the quantity p'; the two bodies will then be subject to the action of a pressure $p-p'$: let us immediately withdraw the two bodies from the compression, and we shall find upon each of them an excess of the contrary electricity greater than that relative to the pressure $p-p'$. It is evident that this plus value is solely attributable to the cessation of the pressure, since the bodies have not ceased to be in contact.

The two fluids developed by pressure are perfectly in equilibrium at the surface of contact; for I have ascertained by very accurate experiments, that neither of the two bodies, during the continuance of the pressure, gives the least sign of electricity. It may be generally asserted that the better conductors bodies are, the greater ought to be the rapidity of their separation, in order to prevent the two fluids from recombining: it is probable that in the case of those bodies which are perfect conductors of electricity, the rapidity of separation ought to be infinite.

The following experiment gives an idea of the influence of the rapidity of separation on the development of electricity. Press an insulated disk of cork on an orange, and withdraw it quickly; it will retain a pretty considerable excess of vitreous electricity: but if instead of withdrawing the disk quickly, it is done more or less slowly, it is regularly perceived that the quantity of electricity developed by the same pressure, diminishes in proportion as the rapidity diminishes, till it becomes imperceptible when that is much abated. We shall hereafter mention an apparatus, by the assistance of which these experiments may be repeated with great accuracy. We shall see that there exists for every substance and for every pressure a degree of rapidity which gives a maximum of electricity.

From these considerations it may be affirmed, that any two bodies whatsoever, whether conductors or non-conductors of electricity, being pressed one upon the other, always enter into two different electric states; but these bodies, after their separation, possess the quantity of electricity due to the pressure, only in proportion as the rapidity of their separation is suitably adapted, that is to say, is sufficient to prevent the recombination of the two fluids.

Caloric appears to have great influence in the phænomena at present under our notice, since it modifies them in a very

peculiar manner. It has long been known, that the more the temperature of a body is raised, the greater is its tendency to acquire resinous electricity by friction with a non-conducting body. Thus, if the temperature of Iceland spar be sufficiently raised, it may be made to acquire resinous electricity by a slight pressure with the disk of cork. The following experiment will also show the influence of caloric in electrical experiments by pressure:—Take a very dry cork, and cut it in half with a very sharp instrument, and press the two parts together by their newly cut surfaces,—each of them will usually acquire an excess of contrary electricity on being withdrawn from compression: it will however be found, sometimes, that they have acquired no excess of electricity, however great may have been the rapidity of their separation. In this case, if the temperature of one of the two disks be raised by warming it slightly at the flame of a taper, both will be immediately electrified by the pressure. Two pieces of Iceland spar of equal temperature are not more electric by pressure; a slight difference of temperature between them suffices to give them the property of becoming electric. May we not conclude from these two experiments, that in two bodies of the same nature, of equal temperature, and in which the state of the particles of the surface is similar,—in two bodies, in short, which are identically the same, no electricity can be developed by pressure. It appears that this must be the case; for if every thing be perfectly alike on each side, there is no reason why one of the surfaces should take the vitreous rather than the resinous electricity, or *vice versâ*: pressure, therefore, cannot change the state of equilibrium of the two fluids which compose natural electricity. If two disks of cork, taken from the same piece, sometimes give out electricity upon pressure, it is probably because the two surfaces are not identically the same: in fact, unless the instrument with which they are cut, separates them with extreme precision, it must follow that the state of the molecules of the surfaces is not the same in both of them. M. Dessaignes had already observed that a glass rod is not excitable when plunged into mercury of the same temperature;—it is the same with sulphur, with amber, and with sealing-wax. There are, however, exceptions; for the same philosopher discovered that paper, cotton, wax, and wool, are always electric by contact, whatever precaution may be taken to equalize their temperature.

If we keep the temperature of one of the disks higher than that of the other, the pressure, as we have just observed, induces upon each piece of cork a different electric state; but if the pressure lasts long enough for equilibrium of temperature

to

to be established between the two bodies, then, on the pressure ceasing, neither of them will have acquired any electricity. It is therefore clear that the development of electricity in this case takes place only during the passage of caloric from the one body to the other; as soon as that has ceased, we see no more electric effects. Thus, then, when two bodies pressed one against the other retain no sensible electricity after compression, before we pronounce on their want of the electric property we must ascertain whether a change of temperature in one of them would not suffice to render them electric.

The hygrometric water which usually adheres to the surface of bodies, sometimes destroys the electric property of pressure: for example, sulphate of barytes, sulphate of lime, mica, &c. must be freed from this water before they are subjected to the experiment; without this precaution, no development of electricity will be obtained: for want of having taken it, some philosophers have concluded that these substances were not electric by pressure. In certain cases it is necessary to attend to the dimensions of the disks: for instance, when Iceland spar is pressed with an insulated disk of metal, if this disk be of a certain size, the development is null; while if it is a millimetre in diameter, the spar immediately acquires an excess of vitreous electricity. Want of polish in Iceland spar entirely changes its electric properties; from being a very bad conductor it becomes a good one; so that it is necessary to insulate it, in order to make it preserve the electricity it has acquired by pressure; its electric sensibility is then considerable.

To recapitulate:—We find that the electric effects of pressure are modified by the temperature of bodies, by the rapidity with which they are separated, by their hygrometrical state, by the state of the particles of their surfaces, &c.

[To be continued.]

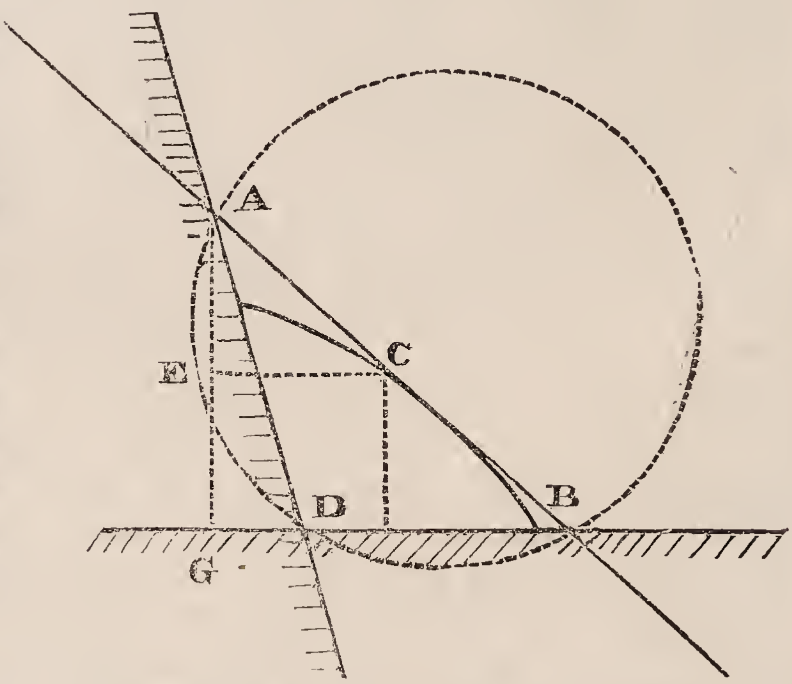
XLIII. *On the Nature of the Curves described by one of the Combinations of* JOPLING'S *Apparatus for describing Curves. By Mr.* THOMAS TREDGOLD.

To the Editors of the Philosophical Magazine and Journal.

IN your last Number a notice was given of a very simple and general method of describing curves, invented by Mr. Jopling. Of this method I propose to take a single case to consider, and one of the easiest; leaving the others to those who are better acquainted with the doctrine of curves, and better versed in the art of analysis.

The case I propose to investigate may be thus stated: Suppose there to be two straight lines on a fixed plane, and two

fixed points in another plane, and let the latter plane be moved, so that while one of the points moves along one of the straight lines, the other point may move along the other straight line: it is required to determine the nature of the line that would be described by a tracer fixed in any part of the moveable plane?



Let AD and DB be the two straight lines on the fixed plane; and A and B the two points on the moveable plane; and, in the first place, suppose the tracing point C to be situate at any point in a straight line passing through the points AB. Put AB $=a$; AC $=na$; AG $=s$; and denote the angle ADG by θ; the abscissa and ordinate of the line described being, in any position of the point C, denoted by x and y.

Now, CE $=na+$GD$-x$; and

$$(na+\mathrm{GD}-x)^2=n^2a^2-(s\mp y)^2.$$

The sign + applying to the case where the tracing point C is out beyond B.

Hence, $\quad x=na+\mathrm{GD}-\sqrt{n^2a^2-(s\mp y)^2}.$

But, $\quad a:na::s:s\mp y;$ or $s=\frac{y}{1\mp n}.$

Also, $\quad \mathrm{GD}=\frac{s}{\tan.\theta}$; consequently $\mathrm{GD}=\frac{y}{(1\mp n)\tan.\theta}.$

Therefore, $\quad x=na+\frac{y}{(1\mp n)\tan.\theta}-\sqrt{n^2a^2-y^2\left(\frac{1}{1\mp n}\mp 1\right)^2}.$

If we suppose the ordinates to be taken parallel to DA, then GD $=0$; and,

$$x=na-\sqrt{n^2a^2-y^2\left(\frac{1}{1\mp n}\mp 1\right)^2}.$$

Hence it appears that the conic ellipse will be described by the tracer, if it be placed any where in the straight line passing through the points AB: excepting when placed so as to describe either a circle or a straight line.

A circle

A circle will be described when $n=\frac{1}{2}$; for then

$$x=\frac{1}{2}a-\sqrt{\frac{a}{4}-y^2}.$$

The tracing point C being, in this case, in the middle between the points A B; the lines A D, D B at right angles, and a will be the diameter of the circle described.

Referring again to the general equation, if we make $n=0$, we have the equation of a straight line, or

$$x=\frac{y}{\tan.\ \theta}.$$

If we describe a circle, to pass through the points A D B, then every point of this circle will describe a straight line passing through the point D; for it is only the reverse operation to describing a circle by an angular point* (see Emerson's Geom. Prop. 41. B. iv.). Therefore the extremities of any diameter of that circle might be taken as the moving points: consequently we can always draw a straight line from a tracing point situate any where in the moving plane to pass through two points in this circle; and lines being drawn from these points to the point D, we may consider these the fixed lines on the fast plane, and our equations become general. Hence we arrive at the conclusion, that any point in the moving plane will describe an ellipse, a circle, or a straight line.

When the tracing point is in the line A B but not between the moving points A B, the principle is identical with that of the common trammel. Also, when the point is in the line, and between the points A B, the mode of describing an ellipse is well known† and interesting to me, because it is the trajectory of the centre of gravity of a beam when it moves between two angular planes by the force of gravitation‡.

16, Grove-place, 13th Sept. 1823. THOS. TREDGOLD.

P.S. In the additions to Buchanan's Essays on Mill-work, just published, I have omitted to state distinctly that the method of finding the least number of teeth for a pinion, so that it may be conducted uniformly by a wheel without part of the action taking place before the teeth arrive at the line of centres, is only approximate. It is founded on the supposition that in a small arc the cosine may be assumed equal to the radius, and the third line of page 52, vol. i., ought to read thus: "But (in small arcs we may, with sufficient accuracy, consider)

* The idea of reversing this operation was suggested by Mr. Jopling, or rather its use in the case we are considering.

† See *Ency. Metho. Amusement des Sciences*, p. 566.

‡ Elementary Prin. Carpentry, art. 38.

sin. A

sin. A &c." the passage between the parentheses being the correction. I am much obliged to the friend who informed me of this omission; for every departure from strict mathematical truth ought to be announced; at the same time such deviations may often be made with great benefit.

T. T.

XLIV. *Postscript to Mr.* J. Utting's *Paper on a Planetary Analogy, page* 119 *of the present volume.*

If the distances of the planets from the sun, and that of the satellites from their primaries, be estimated by the *radii* or *semiaxis major* of the earth's orbit, equal to *unity;* and the velocities of the planets and satellites be taken for one *sidereal year*, the constant quantity thus obtained (viz. $V \times \sqrt{D}$ &c.) will *always* be equal to the circumference of the earth's orbit, *or equal to the circumference of a circle whose radius is unity.*

It has been demonstrated by La Grange that, amid the changes which arise from the mutual actions of the planets, there are two things which remain perpetually the same, namely, the greater axis of the orbit which the planet describes, and its periodic time; so that the *mean motion* of a planet, and its *mean distance*, are *invariable* quantities.

Whence it appears that the velocity of a planet in its orbit, multiplied by the square root of its mean distance from the sun, is not only a quantity resultant for all the planets, but a *constant quantity*, which will *for ever remain invariably the same.*

The quantity produced by $V \times \sqrt{D}$ &c. as above, is equal to the earth's sidereal motion in one year; and if the *radii of the orbit* and *sidereal period* of any other planet, be substituted for that of the earth, the constant quantity thus obtained will *always* be equal to *the circumference of a circle to radius unity* for each planet respectively.

Lynn Regis, Sept. 1, 1823. J. U.

Erratum.—Page 120 lines 21 & 22, *for* =0. *read* =unity.

XLV. *On the Construction of an Air Barometer. By Mr.* Henry Meikle.

To the Editors of the Philosophical Magazine and Journal.

In your Number for January last, Mr. Murray has given the description of a barometer for measuring altitudes, and which in his opinion possesses extraordinary advantages over every other instrument employed for the same purpose. But as these advantages are scarcely enumerated, much less minutely

minutely described, I have somehow either undervalued them or failed in discovering what they are; for the common portable barometer seems still to be the more convenient of the two, especially considering the very troublesome corrections his instrument requires for change of temperature, &c.

In Mr. Murray's barometer, the rise of the mercury, on being carried to a higher station, will, in general, be less than the corresponding depression in the common barometer. Its sensibility will therefore be less; and unfortunately there does not appear to be any means of remedying this defect, which is occasioned by the following causes: The included air, having most likely become colder on reaching the upper station, will be less elastic, and the sinking of the mercury in the cistern, occasioned by its rise in the tube, and by its contraction from cold, must enlarge the space occupied by the air, and still further diminish its elasticity, which will not, therefore, be able to raise the mercury to the proper height. Some inaccuracies, it is true, may be lessened by sliding the tube; but the whole of the requisite corrections could be more easily estimated were the tube fixed; for if the bulk of the glass tube within the cistern be varied, a new source of error will be introduced.

In order to correct this instrument for a change of temperature, we must find—the change in the length of the mercurial column—the change in bulk of mercury in the cistern—and the change in the elasticity of the included air. Indeed, the elasticity of the air would require correction, although the temperature were constant, on account of the variable bulk of mercury in the cistern. It also seems highly probable that the included air, having so large a proportion of its surface in contact with mercury, will be of a temperature intermediate between that of the mercury and the external air, when these disagree.

In its present form, I do not see how a vernier scale can be applied; and without this, the instrument can be of little use in measuring altitudes. Perhaps Mr. M. may hereafter be able to alter it so as to admit of this indispensable appendage. On the whole, I should think his instrument would have been more convenient, though at the same time more liable to fracture, had the tube first branched out from the bottom of the cistern, and then turned round till it stood upright. In this form, a vernier could be applied, and the corrections for temperature, &c. could be as easily computed.

The construction of a more convenient barometer is a subject to which I have often turned my attention; but have not yet succeeded altogether to my wish. The following, however,

ever, is, in my opinion, an instrument greatly preferable to Mr. Murray's, particularly in being more sensible, and only requiring one correction on account of temperature. Its construction is totally different from his; but the principles employed have little novelty to boast of, though, perhaps, they may not have been applied to the same purpose, at least in the same form; and therefore I would hope that a brief account of it may not be altogether unacceptable to your readers.

This instrument, which has some resemblance to an air-thermometer, consists of a hollow ball of glass containing air, from which a vertical tube, open at bottom, descends, and terminates in a cistern of mercury*. The mercury is likewise designed to occupy a part of the tube, more or less, according to the state of the atmosphere. Another tube, equal to the former, and placed close by its side, is also immersed in the quicksilver, though open at top. But in order that the air in the ball and first tube may always be readily brought to the same tension as the air without, the cistern consists of a leathern bag, inclosed in a box, the bottom of which is moveable by a screw precisely as in a mountain-barometer. The mercury in the cistern is, however, open to the external air no where but through the tube, which is open at top.

Now it is manifest, that if the screw at the bottom is turned till the mercury in both tubes stand at the same height, the elasticity of the air within will just balance the weight of the atmosphere: and since in this case the spring of the included air, allowing for change of temperature, cannot sensibly differ from being inversely as its bulk, the space which it occupies will always be inversely as the atmospheric pressure. If, therefore, the tube connected with the ball, or a scale by its side, is graduated, and numbers attached proportional to the contents of the ball, and of that part of the tube which lies above them, these numbers being inversely as the densities, or inversely as the mercurial altitudes in a common barometer, are also ordinates to a logarithmic curve, equal that employed in the usual mode of investigation; and hence the difference of their logarithms has still the same proportion to the difference of elevation†; wherefore these numbers will be equally convenient for the purpose of calculation, as the numbers on a common barometer‡. The mode of applying a vernier,

* Dr. Hook long ago employed air in the construction of his marine barometer; but that instrument is very different from this in various respects.

† Or, more simply, the difference of the logarithms of two numbers is equal the difference of the logarithms of their reciprocals: the logarithms of any number being the arithmetical complement of that of its reciprocal.

‡ These numbers are equally well suited to the very ingenious method of

vernier, and of reading off the observation, being so nearly the same as in a portable barometer, need not here be particularly described; and it is scarcely necessary to remark, that since the surface of the included air in contact with the mercury is so very small, the temperature of the mercury cannot sensibly affect this instrument.

If the air-ball be quite exposed to the air, and be at the same time kept in the shade, it may be presumed that the included air will be at least as near the temperature of the surrounding air as the detached thermometer is; and if so, an attached thermometer may be dispensed with. Indeed, after all the precautions that have been used, it may be questioned whether the thermometer attached to a mountain-barometer may not sometimes differ considerably from the temperature of the mercury in the barometer, especially when the two thermometers themselves disagree.

A difference in the temperatures of the included air at the two stations, will affect the elevation so much more than the same difference would in the temperature of the common barometer, as the effect of heat on air is greater than on mercury. Yet as the temperature of the air seems to admit of being ascertained with greater precision than that of the mercury, it may be presumed that this instrument will not on account of heat be less to be depended on than the mercurial barometer.

If a lighter fluid could be employed in place of mercury, the sensibility of the instrument might be greatly increased; but the evaporation, viscidity, capillary attraction, or some such defect, almost precludes the use of any thing else. The range or scale of this barometer might be made of almost any magnitude, though it is doubtful if its sensibility can be increased quite in the same proportion. Still, when of large dimensions, its sensibility may much exceed that of the common barometer; but a very large instrument would hardly deserve the name of portable. It may however be at least as sensible as the mercurial barometer when only of about half its length.

If the tube connected with the bulb, in place of being cylindrical, were to widen downward, so that the numbers on an attached scale of equal parts might be the logarithms of those already mentioned, the elevation could be obtained with greater facility: but the formation of such a tube with accuracy would be a matter of some difficulty; and unless the divisions are

of computing the elevation given by Dr. Robison, in which no tables are required. The only difference is, that here a correction is to be applied for the temperature of the included air, instead of that of the mercury.

 equal,

equal, it would be still more difficult to apply a vernier to the scale, though it is by no means impossible to do so.

I am, Gentlemen,
Your most obedient servant,

August 21, 1823. HENRY MEIKLE.

XLVI. *Notices respecting New Books.*

Practical Essays on Mill-Work and other Machinery; by ROBERTSON BUCHANAN, *Engineer: the Second Edition, corrected, with Notes and additional Articles, containing new Researches on various mechanical Subjects; by* THOMAS TREDGOLD, *Civil Engineer.* 2 vols. 8vo. pp. 588, illustrated by 20 Plates and numerous Wood-cuts.

THE rapid progress of the art of *constructing Machinery* has rendered works on that subject extremely desirable; they serve at once to record the progress of the art, and to diffuse and improve it. Amongst other able works, Robertson Buchanan's Essays on Mill-Work and Machinery, have contributed in no small degree to make known and improve the constructions of the best proficients in this important art. A second edition of this useful work has just made its appearance, edited by Mr. Thomas Tredgold, who has added a considerable portion of new matter, which to practical mechanicians will be found extremely useful.

As the Essays themselves are pretty well known to the public, we shall confine the remarks we intend to make, to the Editor's additions. The first Essay is on the Teeth of Wheels, wherein is now given, a simple method, by the Editor, of describing Teeth, by arcs of circles, such, as to possess the same advantages, nearly, as the correct theoretic forms: indeed, he has shown that these forms, have the properties which are ascribed to them by writers, only in the imaginary case, when the acting surfaces have no friction. He gives a general investigation of a rule, for ascertaining the smallest number of teeth there should be on a pinion, to produce uniform motion: the calculations which M. Camus had made on this subject, being confined to particular cases, and these not practical ones. The Editor next shows the advantage of forming the teeth of impelled wheels or pinions, so as to resemble the staves of trundles, giving to the impelling pinions or wheels, teeth of a proper figure, to act upon the stave-formed teeth, of the impelled wheels or pinions: by this very simple arrangement, the greater part of the action of the teeth, will occur, after they have passed the line of centres. This important advantage

tage will, when understood, occasion this kind of teeth to be almost universally adopted.

An easy mode of computing the real *radius* of a wheel or pinion, of this construction, is given: but we could have been better pleased, if the manner of finding the real radius, by geometrical construction, had also been explained, the advantages of which mode, in similar cases, the Editor appears to be fully aware of, and therefore we have been surprised that he should here have neglected it. He remarks, that an ingenious rule employed by Mr. Murray, of Leeds, for finding the length of teeth, is founded on the properties of involute teeth, and therefore applicable, to such teeth only. The author had given an erroneous method of forming the teeth of pinions for rack-work, which his Editor has detected, and supplied a rule for finding the real radius of the pinion, and also described the form of the teeth for a rack to move a pinion.

The subject of beveled wheels, has always been esteemed an intricate one; because it has been so treated, as to involve the consideration and description of curves of double curvature; but Mr. Tredgold has been fortunate in discovering a new principle of forming these teeth, which is simple, and very easy of application; and in consequence of the very general use of beveled wheels, in modern machines, and the immense advantage of well-formed teeth for such, this discovery will prove a valuable one. The most important of the properties of involute teeth are pointed out, which show, that they can only be useful in particular cases. In addition to some supplementary definitions of the author, his Editor has now subjoined some interesting definitions of *power*, *force*, *momentum* and *mechanical power*. It is well known that the nature of *force*, was a subject of much discussion about fifty years ago, which has been at intervals revived, up to the present time: the importance of settled and clear ideas on the subject, is of the first importance, in all mechanical inquiries, and in our opinion, the Editor's views are founded in truth.

The horse's power, was by Mr. Buchanan, made the measure of strain, in the parts of machines, and hence his Editor has taken occasion to unfold his own ideas, on the maximum of effect of animal force; and after an interesting inquiry respecting the velocity which corresponds to the maximum effect, he justly gives the preference to Smeaton's measure, of the force of men, as given from the papers of that able engineer, by Mr. Farey junior, the writer of the excellent article Water in Dr. Rees's Cyclopædia. On the strength of the teeth of wheels, the Editor enters into some new investigations, from whence he derives simple and general rules, for guiding the

practical mechanic in fixing their proportions: and he concludes his additions to the first Essay, with valuable remarks, rules and examples for arranging the numbers of the teeth, for wheel-work of mills and other machines.

The second Essay is on the shafts and gudgeons, and the journals (or neck-bearings) of machines, and is accompanied with additions, not less novel nor less important than those we have noticed in the first Essay: the subjects are exemplified by rules, tables and examples, and are treated quite in a new manner, on principles which the Editor has established, in an Essay which he lately published (and of which a second edition is in the press) on "the Strength of Cast-Iron." The second Essay is concluded by one of the most complete tables of the strength of metals that has ever been published, with references to the original works of the experimenters.

The addition of the most consequence, now remaining to be noticed is, on Water-wheels. Practical millwrights had, since the time of Smeaton, ascertained, that overshot water-wheels, do not produce the greatest effect, when the water flows on at their summits, and the advantage was understood, of forming a wheel, so that it might receive the water, at some distance below the summit, as was some time ago mentioned in the Cyclopædic article above referred to, and it was probably in consequence of this mention, that Mr. T. has made it here the subject of investigation, and shown the point, at which the water ought to flow on, so as to produce a maximum effect. He has also determined the velocity, which corresponds to the greatest effect; and shows that each particular height of fall, has its particular velocity, to render the effect a maximum, when the height of the wheel is made to suit the fall. Mr. Smeaton employed only one sized model, he could not therefore obtain a general maximum, and the velocity which he considered the best, is limited to the sized model he used. This shows how careful writers should be, in generalizing from too limited experiments; in fact, it has long been known, in the northern counties, that it was advantageous to give wheels greater velocity, and where (according to Mr. Fenwick) they often have a speed of 9 feet per second, instead of less than 4 feet, as limited by Mr. Smeaton: this interesting subject is closed with very simple formulæ for calculating the power of water-wheels, which will, we think, contribute much, to improve the practical application of this valuable natural power.

There are many subjects of minor importance discussed in the Editor's additional articles and notes (which are all distinctively marked); and on the whole, the work before us, will

will be a valuable acquisition to the library of the mechanical student. The additions are chiefly illustrated by wood-cuts, and there is one new plate: the work is handsomely, although somewhat too widely printed; and the publisher deserves praise, for having placed Mr. Buchanan's work in the hands of an Editor, of a more scientific character than the author, by which we have a combination of views on the same subjects, very favourable to their improvement. At the commencement of the first volume, a concise biographical sketch of the life of the author is given, in the Editor's preface; followed by a justly drawn estimate, of his character and writings.

Zoological Researches in Java and the Neighbouring Islands; by THOMAS HORSFIELD, *M.D. F.L.S. M.G.S.* Five numbers. 1821, 1822. Quarto.

We have too long omitted to notice this valuable work, which has been for some time in a course of periodical publication. Dr. Horsfield, it may be remembered, was engaged by Sir S. Raffles, during the short time that Java remained in our possession, to form a collection of the productions of that island for the East India Company; and he returned to this country three years ago, bringing with him the fruits of his researches, the greatest part of which are now arranged in the Museum at the India-House. The present work is published, we believe, under the patronage of the Honourable Company, and is intended to comprise a selection of the most interesting quadrupeds and birds collected by the author. It is intended to be completed in eight numbers, each containing eight coloured plates, and generally another of anatomical outlines, to illustrate the subjects more fully. The plates of animals are by Mr. W. Daniell, and, with a few exceptions, are in his best manner: those containing the anatomical details are superior to any hitherto published in this country, and reflect the highest credit on the artist, Mr. Taylor, who has given such unequivocal proofs of high excellence in this department: the birds are principally drawn on stone by Mr. Pellitier, and are very good specimens of lithography.

It is not our object to enter into a critical examination of the descriptions which accompany the plates. Dr. H. has proposed several new genera, some of which, we think, rest on good and valid characters; while others have been either already made, or appear to us not so likely to receive general adoption. On the other hand, it should be stated, that the author appears to be actuated by a sincere and zealous spirit of investigation: this is obvious from the course of inquiry he has pursued, and the

the reasons which have guided him; all of which he details to the reader.

From the judgement which Dr. Horsfield has displayed, we are disposed to think that his inquiries would have led, on some occasions, to different conclusions, had his materials for comparison been more extensive: but this is not his fault;—what he has observed, he has minutely described; and these details, if not interesting to the general reader, are useful, and indeed highly valuable to the scientific. That they have not been more extended must be attributed to the lamentable state of the zoological collections in the national Museum, which, instead of being a source of information and of reference to zoological writers, is a meagre gathering of a few half-decayed quadrupeds and moth-eaten birds, exciting the regret of British naturalists, and the contempt of foreigners: but we hope, ere long, for better things; there appears a good and an improving spirit spreading among those who have *power:* we trust it will not slumber, but that all parties will join in placing this portion of our public Museum on the same footing with its other departments.

Having now endeavoured to do justice to the execution of a work, which merits the support of every one at all interested in these pursuits, we shall briefly notice a few of the principal subjects contained in the five numbers before us. In the first Numbers are *Felis Javanensis* and *gracilis*, the latter of which appears to be a new animal; *Viverra musanga; Tapirus Malayanus*, the Malay Tapier; *Irena puella*, (male and female,) a beautiful bird, allied to the Rollers; *Phrenotrix Temia* (the Temia of Vaill.); these two last are placed as new genera: and *Motacella speciosa.*—No. 2. The quadrupeds are, *Mydaus meliceps; Gulo orientalis; Tarsius bancanus;* and *Felis Sumatrana.* The birds consist of *Eurylaimus Javanicus;* a new *Pogardus*, and two others.—The 3d Number has *Tupaia Javanica* and *Tana;* a singular long-armed ape, by the name of *Simia syndactyla*, and *Pteropus rostratus.* The birds are two species of *Falco;* two others of a new genus by the name of *Timalia*, and *Cuculus Xanthorhynchus*, a splendid species.

In the 4th Number are contained, *Semnopithecus* (*Simia* Cuv.) *maurus; Ursus Malayanus; Pteromys genibarbis*, and *Pteropus Javanicus.* We may here observe, that it would perhaps have been better had the author been less sparing of this latter specific name, which implies an *exclusively* local *habitat.* It is true such species have been found only in Java; but it should be remembered, we know scarcely any thing as yet of the zoology of the great islands in the Indian Archipelago; and it is not improbable that many animals, thus designated, will be hereafter

hereafter found in Sumatra, Borneo, or New Guinea. There are four birds in this Number, one of which is proposed as a new genus by the name of *Calyptomena viridis.* The 5th and last part which has reached us contains four quadrupeds, viz. *Nyctinonus tenuis; Mangusta Javanica; Sciurus insignis*, and *Pteromys Lepidus.* The birds introduced are, *Pomatorhinus montanus* (a new genus allied to *Cinnyres*), *Phœnicophaus Javanicus; Scolopax saturata*, and *Muscicapa Indigo.*

Numerical Games: intended for the Improvement of Young Persons. By Thomas Halliday. Hunter, St. Paul's Church-yard, &c.

This small work is evidently the result of much thought, labour, and ingenuity; and we are induced to notice it as being adapted to facilitate the early acquisition of readiness and skill in those numerical calculations which are requisite not only in the usual avocations of commercial life, but also in the pursuits of science. The exercises supply a great variety of recreations, at once instructive and entertaining; and from the rational occupation and exercise which they furnish to the mind, we are disposed to recommend them to the notice of those who withhold from young people games of chance, as being likely to be particularly useful in habituating young persons to *mental arithmetic*, and in cultivating that quickness of apprehension and combination which are the best preparatives for the successful cultivation of the higher branches of knowledge. The games are played with the assistance of appropriate cards and counters; and we can add, from our attentive observation, that children will take great interest and derive much entertainment from them.

Meteorological Essays and Observations, by J. F. Daniell, F.R.S.

Preparing for Publication.

We are very happy to learn that the first number of a Zoological Journal, to be continued quarterly, and edited by Thomas Bell, Esq. F.L.S., John George Children, Esq. F.R. and L.S., James De Carle Sowerby, Esq. F.L.S., and G. B. Sowerby, F.L.S., will appear on the first of January next, and trust that the study of Zoology will be essentially promoted by it.

ANALYSIS OF PERIODICAL WORKS ON NATURAL HISTORY.

Sowerby's Mineral Conchology. Nos. 71, 72, 73, 74.

Since our last notice of this work (vol. lxi. p. 135) the fourth volume has been concluded. The paper by Mr. Farey in

p. 333

p. 333 of our last volume supersedes the necessity of any further detail of its contents. Of the fifth volume, four numbers have also appeared, the subjects figured and described in which are as follows:

No. 71.—Pl. 408, &c. *Crania Parisiensis* with its upper valve: *Plicatula pectinoides*, and *P. inflata* (the former of these has been called a *Placuna* by Lamarck!): *Murex quadratus* and *Calcar: Murex alveolatus, M. defossus*, and *M. sexdentatus: Buccinum labiatum*, and *B. lavatum: Buccinum crispatum*.—No. 72. Pl. 414, &c. *Buccinum tetragonum*, and *B. incrassatum: Buccinum desertum*, and *B. canaliculatum: Murex tricarinatus, M. bispinosus*, and *M. frondosus: Lucina divaricata*, exactly similar to the recent shell: *Mya depressa: Mya gibbosa, Mya intermedia* var., and *M. plicata*.—No. 73.—Pl. 420, &c. *Ammonites Catena*, long celebrated for its loose joints: *Ammonites striatulus, subradiatus* and *cristatus: Venus transversa, V. lineolata, V. elegans*, and *V.? pectinifera*; this is probably distinct from any known genus: *Fusus regularis*, adult: *F. complanatus*, and *F. Lima: Nerita globosa*, and *N. aperta*; this has just appeared in Ferrussac's great work under the name of *N. unidentata: Anomia striata*, generally confounded with *A. Ephippium*.—No. 74. Pl. 426, &c. Two views of *Dolium nodosum*, a very curious and rare fossil: *Cirrus perspectivus*, and *C. depressus: Cirrus rotundatus*, and *C. carinatus: Mitra parva*, and *M. pumila: Trigonia elongata* (a variety of *T. costata*, according to Lamarck).

The seven numbers which have appeared since our last notice of this work, contain 36 new species, besides several that were but imperfectly understood. The shells figured by Brander have received particular attention, and several species, especially of his Volutes, are cleared up. Several former errors are corrected, and the characters of four genera are introduced. The shelly productions of the crag and chalk also have been examined, and several figured; so that probably there is not much left to be done either in those beds or in the London clay.

G. B. Sowerby's Genera of Recent and Fossil Shells. No. 19.

This number contains the following genera: *Sigaretus*, including *Cryptostoma* of Blainville; *Stomatia*, united to *Stomatella; Pileolus*, a new fossil univalve, related to *Nerita; Eburna*, as distinguished from the *Buccinum spiratum* and its congeners, which are usually united to it; *Ranella; Pholadomya*, a new genus of bivalve Shells, of which a single recent species has been lately found, but of which many fossil species have been hitherto described as *Carditæ, Lutrariæ*, &c.

Curtis's Botanical Magazine. No. 439, 440.

Pl. 2419, *Ornithogalum gramineum*, "scapo angulato foliis linearibus altiore, floribus umbellatis, pedunculis erectis, petalis ovatis acutis striatis," raised from seeds from Chili by J. Walker, Esq., and not hitherto described. —*Geranium macrorhizon.*—*Alstrœmeria pulchra*, "caule erecto, foliis lineari-lanceolatis, pedunculis sub-umbellatis involucratis trifloris, pedicellis tortuosis, petalis exterioribus obcordatis mucronatis;" from Chili.—*Pulmonaria mollis*, from the botanic garden at Bury St. Edmund's.—*Erysimum lanceolatum* β. minus.—*Œnothera tenella*, from Chili.—*Hyacinthus amethystinus*.—*Spiræa bella*, "caule fruticoso, foliis ovatis acutis argute serratis subtus tomentoso-albidis, paniculis terminalibus foliaceis."

Pl. 2427. *Magnolia acuminata.* Catesby's figure appears to have been from *M. glauca*, and not from this plant. *Ixora rosea* Roxb. Fl. Ind. *Vitis riparia*, mascula. *Pyrus Amelanchier:* Mr. Lindley has proposed

Amelanchier

Amelanchier as a distinct genus, containing, with this plant, *Pyrus botryapium, ovalis* and *cretica;* while on the contrary Sir J. E. Smith has thought it best to reduce the whole order in which it stands to the two genera, *Mespilus* and *Pyrus*. *Erythrina caffra*, native of Southern Africa, flowered for the first time in this country last year; and now figured, as is supposed, for the first time. *Arum Italicum*, frequently confounded with *A. maculatum*. "It was in this species (adds the Editor) that M. Lamarck observed an extraordinary degree of heat, amounting almost to burning, in the spadix, at a certain epoque, probably that when the fecundation of the germens takes place. This high temperature continues only for a few hours, and when several spadices come from the same root, the heat is evolved from each, in succession, as they arrive at the proper epoque, while the rest remain at the same temperature as the surrounding atmosphere. This observation is said to have been confirmed by Desfontaines.

"We are not informed, however, that the fact was proved by the thermometer; and, if not, it is possible that some pungent vapour might occasion the sensation of heat in the fingers, without really increasing the temperature of the surrounding air. We hope some of our readers may be induced to attend to this curious phænomenon."

The Botanical Register. No. 103.

With this number are given the descriptions of the following plants, the figures of which were contained in Nos. 100 and 101:—

Plate 711. *Physica capitata;* belonging to the Natural Order of *Rhamneæ* described by Mr. Brown in Flinders's Voyage, 2, 554, where upwards of 30 plants of this order are said to be found in Terra Australis.—*Lonicera flexuosa;* native of China, said to be quite new to our collections.—*Marica cærulea*, a newly observed species from the Brazils, very near to *M. Northiana*.—*Amaryllis Belladonna*, from Southern Africa, long confounded with *Aquestris*, a West Indian plant.—*Pancratium australasicum*, from the newly explored inland parts of New South Wales, where it was lately discovered by Mr. Cunningham, the zealous investigator of the natural history of those regions.—*Tabernæmontana laurifolia*.—*Scabiosa Webbiana*, gathered on the summit of Mount Ida by Mr. Barker Webb in October 1819.—*Tropæolum peregrinum*.—*Amaryllis maranensis*, the *Hippeastrum stylosum* of Mr. Herbert in Curtis's Mag.—*Calanthe veratrifolia*: this is a genus separated from *Limodorum* and *Bletia* by Mr. Brown.—*Acacia Lambertiana*, a new Mexican species from the collection of Don José Pavon: the description by Mr. Don.—*Brachystelma tuberosum*; a genus of the Nat. Ord. of Asclepiadeæ established by Mr. Brown.—*Calceolaria corymbosa*.—*Amaryllis candida*, sent to the Horticultural Society in 1823 from Peru.

XLVII. *Proceedings of Learned Societies.*

HORTICULTURAL SOCIETY OF LONDON.

July 1.—THE Silver Medal was presented to James Cowan, Esq. for his attention to the objects of the Society in sending a valuable collection of seeds and bulbs from Peru for the Garden of the Society.

The following communications were read:

Directions for cultivating the Sugar Cane, with Observations on the Species and Varieties of that Plant. By Mr. George

George Caley, Corresponding Member of the Society, and Curator of the Botanic Garden at St. Vincents.

On the Cultivation of Parasitical Plants. By the Hon. and Rev. William Herbert, D.C.L. F.H.S. &c.

July 15.—The following communications were read:

On the Cultivation of tender Roses by budding on the Musk Cluster Rose. By John Williams, Esq. Corresponding Member of the Society.

On the Cultivation of Dahlias. By Mr. John Mearns, F.H.S.

An Account of a new Variety of Apple. By M. André Thouin, Foreign Member of the Society.

August 5.—The Silver Medal was presented to Mr. George Washington Jones, for having first introduced into this country Plants of the *Aracacha* from South America, and for presenting the same to the Society.

The following communications were read:

An Account of a Steam-Apparatus erected at Barton Cottage, Hampshire, by Mr. John Hague; communicated by John Dent, Esq. M.P. F.H.S.

On the Cultivation of the *Arachis hypogea.* By Mr. John Newman, Gardener to the Hon. Robert Fulke Greville, F.H.S.

August 19.—The following communications were read:

On the Cultivation of tender Plants in the open Air. By Mr. Nathaniel Shirley Hodson, Corresponding Member of the Society.

On the Cultivation of Citrons. By Mr. Archibald Craig, Corresponding Member of the Society.

September 2.—On the different Modes of increasing Solar Heat on the Surface of Garden Walls, &c. By John Williams, Esq. Corresponding Member of the Society.

September 16.—The Silver Medal was presented to Edward Nicholas Bancroft, M.D. of the Island of Jamaica, for his attention in sending to the Society plants of the *Aracacha.*

Also to William Atkinson, Esq. F.H.S. for having produced the new variety of Strawberry called the Grove End Scarlet Strawberry.

Also to Mr. Peter MacArthur, Corresponding Member of the Society, Gardener to Alexander Baring, Esq. at the Grange, Hampshire, for his skill in the cultivation of fruits, as evinced by the specimens exhibited by him at the meetings of the Society on the 5th and 19th of August and 2d of September.

The following communications were read:

On the Propagation and Growth of the *Yucca filamentosa.* By Mr. Folkes, Gardener to Sir Everard Home, Bart. F.H.S.

On a Method of treating Dwarf Standard Apple- and Pear-Trees. By Peter Rainier, Esq. Captain R.N. F.H.S.

ROYAL

ROYAL ACADEMY OF SCIENCES OF PARIS.

April 28.—The following memoirs were received:

On an Instrument for Measuring Angles and Lines; by M. Hurtel.—On the Binomial Theorem; by Mr. John Walsh.—On the Existence of Hydrocyanite of Iron in Wine; by M. Julia.—Meteorological Observations at Alais; by M. d'Hombres Firmas.—Additional notes to the former communication of M. de la Borne on Voltaic Electricity.

The conclusion was read of the Report on M. Bertrand Roux's Geological Description of Puy-en-Velay, and particularly of the Valley in which that city is situated:—the work received the approbation of the Academy, and was ordered to be published. M. Geoffroy St. Hilaire read a Memoir entitled General Considerations on the Sexual Organs.—M. Desfontaines made a Report, in the name of a Commission, on the Memoir of M. Paulet on the Synonymy of the Plants of Theophrastus:—this work required the research of one well skilled both in the knowledge of plants and in the learned languages; and although it cannot be said that the author has in all points been equally successful, yet his work will be of great use to those who read Theophrastus.

May 5.—A Memoir was received from M. Turban, on the Internal Navigation of Paris; from M. de la Borne, on the Influence of the Multiplication of Bars in the Circuit of Doctor Seebeck; and from M. Metternich, a complete Theory of Parallel Lines.

M. Feuillet was unanimously elected Librarian in the room of the late M. Charles.

M. Brongniart read a Report on the Memoir by M. Becquerel relative to the Plastic Clay of Auteuil, which received the full approbation of the Academy.—M. Gaymard, one of the naturalists who accompanied M. Freycinet round the world, read a Memoir on the Form of the Sculls of the Papous; and MM. Pelletier and Dumas, a Memoir on the Elementary Constitution and some characteristic Properties of Vegetable Alkalis.

May 12.—M. Fresnel was unanimously elected a Member of the Academy.—The Prize for an Essay on Animal Heat was awarded to M. Despretz of the Polytechnic School.—It was the opinion of the Commission that the physiological Prize founded by M. de Montyon should be divided between M. Foderà, author of a Memoir on Absorption, and M. Flourens, author of a Memoir on the Functions of the Nervous System*. —M. Edwards read a Memoir on the Production of Carbonic Acid in Respiration.—M. de Lalande's two Medals were given

* See Philosophical Magazine, vol. lxi. p. 114.

 at

at the recommendation of the Section of Astronomy, the one to M. Rumker, and the other to M. Gambart, jun.

May 19.—A Memoir by M. Hill was received relative to certain new Means of Producing Sound; also one by M. Marcel de Serres, entitled Observations on the Human Bones discovered in the Fissures of Secondary Strata, and in particular on those which are found in the cavern of Durfort in the Departement du Gard.—M. Poinsot read a Memoir on the Analysis of Angular Sections; and M. Gay-Lussac read his Reflections on Volcanos*.

May 26.—M. Cauchy read a Memoir on the Determination of Definite Integrals; and MM. Prévost and Dumas read their Memoir on the Decomposition of Urinary Calculi in the Bladder by means of the Voltaic Pile.

M. Prony made a very full Report, in the name of a Commission, on the work of MM. Clapeyron and Lamé relative to the Stability of Arches. It appears that these young engineers had been anticipated in the discovery of the fundamental bases of the theory, by M. Audoy, chef de bataillon of engineers. Their work is not the less worthy of praise: the geometrical construction which they give of the point of rupture is curious; and the analysis is conducted with skill and elegance.

June 2.—At this sitting were read M. Fourier's Eloge on Delambre; a Memoir by M. Magendie on some recent Discoveries relative to the Functions of the Nervous System; M. Cuvier's Eloge on M. Haüy; and Considerations on the Commercial Strength and Public Works of France and England, by M. Dupin.

June 9.—A paper on Mathematical Analysis, by Mr. Walsh, was received.—M. Cuvier read a Memoir, "*Sur une Phalange onguéale fossile*," which may be presumed to have belonged to an unknown toothless animal, probably a gigantic species of pangolin.—M. Œrsted was elected to fill the vacancy among the Corresponding Members; the other candidates heing MM. Chladni, Seebeck, Brewster, Amici, and Gilbert of Leipsic.

M. A. St. Hilaire read a first Memoir on the Ginobasis.

June 16.—A Memoir was received from M. Lambert on Symmetric Polyhedrons.

M. Becquerel read a Memoir on the Development of Electricity by the contact of two portions of the same metal of a sufficiently unequal temperature. Also, M. Cuvier read a Memoir entitled Observations on a singular Alteration of some Human Sculls.

* See page 81 of our present volume.

M. Ampère

M. Ampère presented to the Academy an Instrument for Measuring the Intensity of the Electro-dynamic force, in determining by experiment the duration of the oscillations which are produced, at various distances, in a circular moveable conductor, by the action of two semi-circumferences forming part of a Voltaic circuit.

Mr. Walsh had addressed to the Academy a fresh note on what he formerly denominated the *binominal calculus*, and which he now wished to call the Irish calculus (*calcul d'Irlande*), Mr. Walsh's country. The report now read by M. Cauchy on this subject was not more favourable than his reports on the preceding memoirs of the same author. M. Lassaigne read his Observations on the Existence of Cystic Oxide in a vesical Calculus from a Dog, and an analytical Essay on its elementary Composition.

June 23.—M. de Humboldt gave a detailed account of the new work on the last Eruption of Vesuvius, published by MM. Monticelli and Covelli *; and he communicated the results of the measurements which he made shortly after that event.

M. de Freycinet communicated a letter written by M. Duperrey, and dated from La Conception, in Chili, the 24th of January last. M. Duperrey announces the transmission of the Magnetic Observations, and of those on the Pendulum, which he made at the various places at which he touched during his voyage.

XLVIII. *Intelligence and Miscellaneous Articles.*

PROPOSED ESTABLISHMENT OF A METEOROLOGICAL SOCIETY.

THE science of Meteorology, we understand, is likely to receive, in a short time, the powerful aid of a Society expressly devoted to its cultivation. A meeting will be held on the third Wednesday in October, at the London Coffee-house, Ludgate Hill, at 8 o'clock in the evening, for the purpose of taking the subject into consideration, at which a number of scientific gentlemen, attached to the science, are expected to attend: and we hope their example will be followed by all who are interested in Meteorological pursuits.

MISSION TO THE INTERIOR OF AFRICA, FOR THE DISCOVERY OF THE COURSE OF THE RIVER NIGER.

We have the greatest satisfaction in announcing that our

* See page 90 of our present volume.

three

three enterprising countrymen, Dr. Oudenay, Major Denham, and Lieut. Clapperton, who left London on the above interesting and hazardous expedition, under the authority of Government, in 1821, arrived at Bornou, in the centre of the continent of Africa, in February last, and were exceedingly well received by the Sultan of that kingdom.

The Doctor (an eminent professor from one of the Scotch Universities) is to remain at Bornou as British Vice Consul, while the other parties pursue their inquiries as to the course of this long-sought river. All the parties were then in good health and spirits, though they have all at times suffered severely from the rigours of the climate. Their route has been over dreary deserts of 15 or 16 days journey in length; but their undiminished zeal and ardour in the service augur well of their ultimate success. The fatigue and privations they have suffered have been extremely great. They are, however, borne with scarcely a complaint or murmur; and we sincerely hope such exertions may be rewarded by the complete discovery of their object of research. At all events, the public will hereafter be gratified with many interesting particulars, before unknown, of this curious and unexplored region of the world.

CAPTAIN SABINE'S EXPEDITION.

A letter from an officer on board his Majesty's gun-brig the Griper, on her voyage to the North Pole, dated Hammerfats Bay, Norwegian Lapland, June, 1823, says—"We arrived here safe on the 2d instant. On the 24th May we passed the arctic circle, and experienced some difficulty in finding Hammerfats Bay, as the whole land is one continued chain of islands along the coast, and but imperfectly laid down in the charts. We enjoy excellent health, and are extremely comfortable. The weather is now getting better, as summer is rapidly advancing, and we have a continuation of day-light all the twenty-four hours, the sun never sitting below the horizon. The island is about 24 miles in circumference, and five or six in breadth, and gives name to a small town of about 30 or 40 wooden houses, containing about 200 inhabitants. Captain Sabine has all his instruments on shore to commence his operations. We expect to remain here 12 or 14 days, when we proceed to Spitzbergen. Should we return this winter, the Captain proposes calling at Drontheim, the capital of Norway."—*Inverness Journal.*

ORIENTAL MANUSCRIPTS.

The celebrated philologer Rask, in the course of the journey

ney into the East which he has prosperously accomplished, has made a most valuable addition to the literary treasures of the university of Copenhagen, in a collection of one hundred and thirteen manuscripts in various oriental languages, and of great antiquity. Of these, thirty-three belong to the Persic literature, including very ancient copies of the Zendavesta. The rest relate to ancient Indian literature, and are written in the ancient Indian and Malabaric dialects.—*Revue Encycl.*

NEW VOYAGE OF DISCOVERY.

Portsmouth, Sept. 22.—We have just been visited by a navigator of great celebrity, who is going on his third voyage round the world,—Captain Otto Von Kotzebue, who accompanied Captain Krusenstern, and afterwards made a voyage to the South Sea, and North East coast of America, in a small vessel fitted out at the expense of that munificent patron of science Count Romanzoff. On the present occasion he is sent by the Russian Government, and nothing has been neglected to insure the success of the voyage. The ship was built last winter expressly for the voyage. She is a corvette, called the Enterprise, carrying 24 guns, and manned with a crew of 80 men and 13 officers, all volunteers from the Imperial navy; she has on board two physicians, both well versed in Natural History—one of them is Dr. Eschscholz, who accompanied Captain Kotzebue on his late voyage. The Astronomer is Mr. Preiss; Mineralogist, Mr. Lintz; and professed Naturalist, Mr. Hoffman. These gentlemen are all from the University of Dorpat. Immediately on his arrival, Captain Kotzebue went to London to receive the astronomical instruments and the chronometers, which had been previously ordered by the Imperial Government for this expedition. The astronomical instruments are made by the celebrated Troughton, and by Jones instrument maker to the Admiralty. The chronometers are by Parkinson and Frodsham, whose improvements in these machines have obtained much well-merited praise, since their superiority has been so fully proved in several of the late scientific voyages, especially Captain Parry's to the Polar Sea, and Captain Sabine's to the coast of Africa. As the object of this expedition is said to be not so much for new discoveries, as to make accurate surveys, and most strictly to determine, by astronomical observations, the real situation of many important points, we cannot but applaud the judgement and liberality of the Imperial Government in applying to the above eminent artists for the numerous instruments required for the

full

full attainment of the object proposed. Captain Kotzebue's destination is to Rio Janeiro, round Cape Horn to Kamtschatka, where he will find further instructions, which are to be forwarded over-land through Siberia.

PRUSSIAN TRAVELLERS.

Drs. Ehrenberg and Hemprich, Prussian naturalists now travelling in Egypt, are not expected, as some journals have stated, to return immediately to Europe. On the contrary, they were, according to the last accounts from them, about to avail themselves of the assistance afforded by His Majesty for a new expedition. Their plan, as described in a letter dated Suez, June 8, is as follows: In the first place to proceed along the coast of the Red Sea, making their longest halt at Tor and Abaka. They will afterwards embark for Mocca, whence they will make excursions on the coast of Abyssinia, and in the islands situated near Ral and Nandel. Hence they mean to proceed to Suakin, and, if circumstances permit, to penetrate again into Nubia and Sennaar, to examine those fertile countries with which they had acquired a slight acquaintance on their former journey, but only by skimming the frontiers. They wish to return to Cairo by Cosseyr and Ginch. We have already received from them thirty large packing-cases, containing valuable articles collected during their voyage in Nubia, and which furnish most interesting information on countries hitherto very little known. What curiosities they have since collected have been embarked for Trieste, and we expect to receive them before the end of the present year. From the researches of these zealous and intelligent travellers, we expect important results for the study of natural history and geography.—*Berlin Paper.*

VIOLENT STORMS, AND WATER-SPOUTS.

On August 26, at three o'clock in the afternoon, the sudden heat of the atmosphere announced an approaching storm, which showed itself coming from the S.E. over the village of Boncourt (Canton of Anet), and not far from thence a remarkably large water-spout made its appearance. Its base touched the earth, and its summit was lost in the clouds. It was formed of a dense dark vapour, and flames frequently darted through its centre. In its course onwards, it tore up or broke the trees for a space of a league, destroying between seven and eight hundred trees, and at length burst with vast impetuosity on the village of Marchefroy, destroying in an instant one half of the houses. The walls were shaken to their foundations, and crumbled down in every direction; they were torn off and

and split, and the pieces carried half a league away by the force of the wind. Some of the inhabitants who remained in the village were knocked down and wounded; those who were at work in the field, fortunately the greater number, were also thrown down by the violence of the storm, which destroyed the harvest and wounded or killed the beasts. Hail-stones, as big almost as a man's fist, stones, and other bodies, showered down by this impetuous wind, wounded several individuals very severely. Waggons heavily laden were broken in pieces, and their burdens dispersed. Axle-trees capable of supporting the weight of eight or ten tons were broken, and large wheels were carried two or three hundred paces from where the storm found them. One of these waggons, almost entire, was even carried over a brick-kiln, some portions of which were carried to a considerable distance. A steeple, several hamlets, and isolated houses, and new walls, were blown down, and other villages were considerably damaged. The spout occupied about 100 toises at its base, if we may judge from the durable and disastrous marks it made in its progress.—*Journal des Debats.*

On the 19th of August a terrible storm passed over Brussels, which did great damage in Zeilich and other places. A water-spout that accompanied it broke twenty large trees within six feet of the ground, which blocked up the road so as to stop the diligence from Antwerp. The storm raged chiefly in the direction from Aelst to Mechlin. Above 100 trees were snapped asunder, or torn up, at the corner of a small meadow; and between Mazeendeel and Steinhuffel, several thousand trees of all kinds and sizes have been thrown down, or stripped of their foliage. Of course, every thing in the fields and gardens is destroyed, and the corn may be gathered up as on a thrashing-floor. Hailstones as large as a hen's egg were picked up, and pieces of ice several inches long and an inch thick.

EARTHQUAKES.—ERUPTION OF A VOLCANO IN ICELAND.

A shock of an earthquake was felt at Madras on the 2d of March, extending through the Nilgherry and the country in that direction, as well as generally along the coast. The shock was also perceived in Travancore, but twenty minutes later than at Madras, and also in the island of Ceylon.

Accounts from Iceland, of the 16th of August, say, that the volcano of Kollugcan, in that island, which had been quiet for 68 years, made a terrible eruption on the 26th of July last, accompanied by an earthquake; enormous blocks of ice were detached from the summit of the mountain; a great extent of country was laid waste; but fortunately no

 lives

lives were lost. Ships which were 20 leagues distant in the open sea, were covered with volcanic ashes. There were three distinct eruptions, each very violent.

NEWLY DISCOVERED MINES IN FRANCE.

There have lately been discovered in the environs of Confolens, in the department of the Charente, and at Melle, in the department of the Deux Sevres, several mines of zinc and lead. The presence of a great mass of metallic matter has been ascertained by a Company formed to make experiments. Sulphat of zinc and lead, in combination with silver, have been found, and submitted to analysis by the most distinguished chemists of Paris: it has been from 3 to 3½ ounces of silver to the old quintal. Cadmium, a metal lately discovered in Hungary, has been detected in these minerals; the uses to which it may be put are, however, not yet very well known. These mines are situated in a country where fuel is abundant and cheap. The Charente and the Vienne flow close by the spot where it is purposed to place the machines; and the high-road is not far off. Some specimens of the produce of these mines are now to be seen in the Louvre; and some rich capitalists propose to work them on a grand scale. —*Courier François.*

FORMATION OF PRUSSIC ACID BY THE IGNITION OF A CARBONACEOUS SUBSTANCE WITH NITRATE OF BARYTES.

In our last number, we gave an extract from Silliman's Journal, respecting the production of cyanogene by the action of nitric acid upon charcoal: the subject has recalled to our remembrance a notice found among the late Mr. Gregor's papers by Dr. Paris, and published by him in the first volume of the Transactions of the Geological Society of Cornwall, in which certain effects are described that must have resulted from a similar action; and as we believe the notice in question to be little known, it may be useful to republish it.

"The species of coal known by the name of culm," says Dr. Paris, "*Glanz Kohle*, is imported, on account of its purity, for the purpose of smelting tin. Mr. Wm. Gregor informed me, shortly before his death, that he had observed amongst the heaps of this coal lumps of a much more dense texture, and which were perfectly uninflammable. In order to decompose it, he powdered it, and added twice its weight of *nitrate of barytes*, and subjected it to heat in a platina crucible; when, to his great astonishment, a violent detonation took place, accompanied with a copious evolution of *prussic acid* vapours; and, upon examination, he found the residue in the crucible to consist

consist of the *prussiate* and *carbonate of barytes.* Since Mr. Gregor's death I have examined his chemical memoranda, and am thereby enabled to extract the following facts. From different experiments the specific gravity of this substance appears to be 1.627. Fifty grains of *the coal* were mixed with 200 of *nitrate of barytes*, reduced to powder, and placed in a platina crucible, which was set in a common fire: before the crucible became red hot, a violent detonation took place, with the disengagement of a brilliant light and vivid heat, which rendered the crucible and its cover red hot; a porous light greyish mass, mixed with black streaks, remained, which smelt of prussic acid. This was separated from the crucible and pulverized, when it was introduced into a mattrass. Muriatic acid operated upon the powder, and a considerable quantity of an elastic fluid was disengaged. The solution assumed a dark blue colour, and a very light powder was suspended in it, resembling *Prussian blue.* It was poured off with the fluid, and the remainder was a portion of the undecomposed mineral, which, when dried, weighed $23\frac{3}{4}$ grains. This residuum was mixed with 100 grains of *nitrate of barytes*, and treated as before, when a detonation again took place, but with less energy, a greyish mass remaining, which was treated with muriatic acid as before: there was now no blue powder separated, but the lixiviated mass became opaline. The undissolved residuum now weighed $15\frac{7}{8}$ grains: this was again mixed with 50 grains of the *nitrate of barytes;* a brisk detonation and vivid flame were produced. In this case the vessel was exposed to a stronger heat than before; and on the addition of muriatic acid, a blue powder was again separated, when the undecomposed residue was edulcorated and dried. It weighed $8\frac{1}{4}$, which was mixed with 40 of the nitrate, with the same phenomena, and the same separation of a blue coloured powder, by the effusion of muriatic acid. The residue now weighed only $2\frac{1}{2}$ grains: this underwent a similar treatment; and after this, as not one grain remained undecomposed, it ceased to be an object of experiment. A strong smell of prussic acid accompanied the detonation."—*Trans. Geol. Soc. of Cornwall, vol.* i. *p.* 229.

MR. JOPLING'S APPARATUS FOR DESCRIBING CURVES.

The following testimonial has been published of the utility of an apparatus invented by Mr. J. Jopling, architect, for generating Curves of several divisions of his system.

" August, 1823.

" We the undersigned have seen Mr. Joseph Jopling's newly invented apparatus for the organical description of

 curved

curved lines, and have also seen its mode of operation, and have inspected a great variety of curves which have been described by means of it. We have no hesitation in saying, that we regard this apparatus as most simple and ingenious, capable of producing, with the utmost facility, an indefinite variety of curves, comprehending those which have been the subject of mathematical research, and numerous others, which cannot fail to be of great utility in naval architecture, in the ornamental departments of civil architecture, and in the formation of patterns in the imaginative regions of the arts. To mathematicians, the use of this apparatus will suggest a variety of inquiries in reference to new and curious curves, whose properties have not as yet been investigated; while to architects, shipwrights, engravers, and many others, it will be found subservient to the most fertile and interesting applications."

(Signed,) OLINTHUS GREGORY, LL.D. Professor of Mathematics in the Royal Military Academy.
S. H. CHRISTIE, M.A. of the Royal Military Academy.
ARTHUR AIKIN, Secretary to the Society of Arts, &c.
THOMAS TREDGOLD, Civil Engineer.

The Apparatus may be had at Mr. Taylor's Architectural Library, Holborn; at Mr. T. Jones's, Philosophical Instrument-maker, Charing Cross; and at Mr. Jopling's, 24 Somerset-street, Portman-square.

A friend who has seen the machine assures us that nothing can be more simple, more easily managed, or more free from any thing to obstruct the operator from seeing the describing point. To engravers it seems likely to be an invaluable acquisition. It describes all species of the conchoidal, elliptic, cardioidal, and many other species of curves; every section of a ship, so that they shall range; arches of every form that can be desired: and it may be successfully applied to describe an immense variety of patterns, which you can make perfectly symmetrical, identical, or vary in any manner.

An account of the principles on which the apparatus is constructed, is given by the inventor in a small work (sold by Taylor, Holborn,) entitled "The Septenary System of Generating Curves by continued Motion."

QUESTION BY JOHN HAMETT, ESQ.

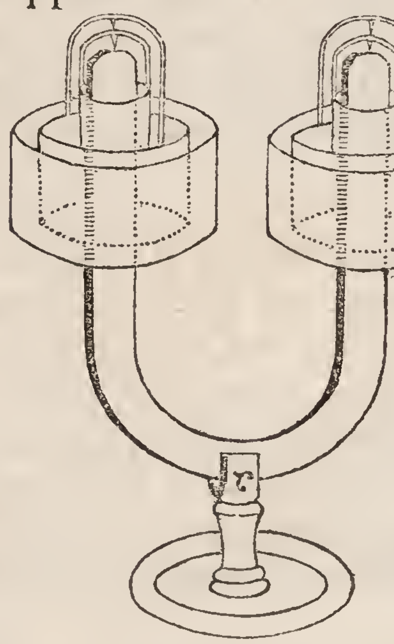
"In the construction of Pythagoras's theorem, lines are drawn from the acute angles of the right angled triangle to the opposite angles of the squares described upon the sides containing the right angle; and a line is drawn from the right angle parallel to either side of the square described upon the side subtending the right angle. Now, as it so happens that these three

three lines intersect in one common point within the triangle, and that this circumstance of intersection has not been demonstrated in any of the books of geometry with which I am acquainted, I will thank any of your ingenious Correspondents to give a genuine Euclidian demonstration of this, without bringing in to his aid any proposition beyond the 47th itself."

J. Hamett.

ELECTRO-MAGNETIC ROTATION.

The electro-magnetical revolving cylinders of zinc and copper as first contrived by M. Ampère, and improved by Mr. Marsh of Woolwich, certainly rank among the most pleasing instruments for exhibiting electro-magnetic rotation that have yet been contrived. It gives us pleasure to introduce to the public a still further improvement upon this apparatus.



Mr. Sturgeon, a pensioned artilleryman of Woolwich, who has successfully devoted himself to scentific pursuits, has constructed the apparatus with two sets of revolving cylinders, one suspended on each pole of an inverted horse-shoe magnet, as the annexed figure illustrates. Upon the usual insertion of the diluted nitric acid the two sets of cylinders simultaneously enter into rotations in a very interesting or striking manner. This form of the magnet gives the advantage of increased power on a reduced altitude, and the proximity of the poles materially augments the rotation of the opposed cylinders. The effect is the most pleasing we have ever seen, and was witnessed at the house of Messrs. Jones, opticians, Holborn.

FASCINATION.

A very singular fact occurred at Manchester (U.S.) a few days since. As Mr. Samuel Cheever was at work in the field, his attention was arrested at the sight of a number of fowls, with heads erect, and wings extended, standing in a circular manner. On going near to ascertain the cause, he saw a large black snake of five feet in length within the circle, and his squamous head elevated eight or nine inches above the surface of the earth, while his posterior parts remained in a spiral form. And so complete was the fascination, that Mr. Cheever was under the necessity of getting a pole to disperse the fowls, in order to kill the snake, in which he happily succeeded.—*Salem Register*, *Aug.* 2.

Meteoro-

Meteorological Observations at Great Yarmouth, by C. G. Harley, Esq.

[Continued from vol. lxi. p. 399.]

	Days.										Thermom.			Rain.
1823.	Dry.	Wet.	E.	SE.	S.	SW.	W.	NW.	N.	NE.	Low.	High.	Med.	In.
May	19	12	1	9	4	7	3			7	50	65	58	$1\frac{5}{8}$
June	12	18	1	3	2	6	2	2	5	9	54	71	61	$1\frac{4}{8}$
July	9	22		5	3	15	2	2	1	3	56	76	65	$2\frac{6}{8}$
August	9	22			6	15	6		2	2	59	76	66	$2\frac{6}{8}$

Remarks.—The temperature of May 13·29 above the mean of May for the last 29 years. June 2, 11·29 below. July 1, 18·29. August 1, 10·29. The variations of temperature have been unusually great; the thermometer frequently varying from 15 to 20 degrees in 10 or 12 hours. White frosts in the nights of July 7th, 17th; August 9th, 10th, and 13th.

An exact resemblance between July and August in the number of dry days and wet days, and in the quantity of rain; a fact which has not occurred for 29 years in two adjoining months.

REMARKABLE METEOR.

May 23d, at 10 o'clock at night, a luminous meteor was observed at Kiel in Denmark. It was seen almost at the same time at Copenhagen, which is 60 miles from Kiel. This will give some idea of its size and of its velocity, which was apparently not very great. At Kiel it seemed to take a direction from S.E. to N.E. and to have an elevation of 30 degrees. It was visible for 10 seconds. As it disappeared, it threw out a volume of sparks, and left a luminous track in the sky.

LECTURES.

Guy's and St. Thomas's Hospitals, Southwark.—The annual Course of Medical and Scientific Instruction at these Hospitals will commence early in the ensuing month of October, when separate Courses of Lectures will be delivered on the following subjects; viz.

Practice of Medicine, Pathology, Therapeutics, and Materia Medica, by Drs. Cholmeley and Back, Physicians to Guy's Hospital.

Principles and Practice of Chemistry, by William Allen, Esq. F.R.S., Dr. Bostock, F.R.S., and Arthur Aikin, Esq.

Experimental Philosophy, by William Allen, Esq. F.R.S., and John Millington, Esq. Prof. Mech. Phil. Roy. Inst.

Midwifery and Diseases of Women and Children, and on Physiology, by Dr. Blundell.

Anatomy and the Practice of Surgery, by Sir Astley Cooper, Bart., and Mr. Green.

Structure and Diseases of the Teeth, by Mr. Thomas Bell.

Medical and practical Botany, by Dr. Bright.

A Course of Chemical Lectures will be delivered in the season.—Particulars to be had of Mr. Stocker, Apothecary to Guy's Hospital, who enters Pupils to all the above Lectures.

Obituary.—M. Brequet.

The funeral of M. Brequet, the celebrated watchmaker, took place on Sept. 18th. He was followed by a great number of men celebrated in the sciences and arts, to the cemetry of *Père le Chaise*. There were deputations from the Academy of Sciences, the *Bureau de Longitude*, the Council-general for Manufactures, and from the Jury to decide on the Exposition of the Products of Industry; of all which M. Brequet was a member. M. C. Dupin, in the name of the Academy, M. Arago in the name of the *Bureau*, and M. Ternaux in the name of the Council, expressed successively the regret of these bodies. They paid a proper tribute to the talents and virtues of M. Brequet, who reckoned among his friends the persons who were charged to express the universal regret which was produced by the unexpected death of an artist whose vigorous old age seemed to promise a much longer career.

LIST OF NEW PATENTS.

To Benjamin Rotch, of Furnival's Inn, London, esq., for his improved fid for the upper masts of ships and other vessels.—Dated 21st of August, 1823.—6 months allowed to enrol specifications.

To James Surrey, of Battersea, Surry, miller, for his method of applying heat for the producing steam and for various other purposes, whereby the expense of fuel will be lessened.—4th Sept.—2 months.

To William Woodman, of York Barracks, veterinary surgeon of the 2d Dragoon Guards, for his improved horse's shoe, which he denominates the beviled heeled expanding shoe.—11th Sept.—2 months.

To Bryan Donkin, of Great Surry-street, Surry, engineer, for his invention on the means or process of destroying or removing the fibres from the thread, whether of flax, cotton, silk, or any other fibrous substance composing the fabrics usually termed lace net, or any other denomination of fabric, where holes or interstices are formed by such thread in any of the aforesaid fabrics.—11th Sept.—2 months.

To John Hughes, of Barking, Essex, slopseller, for certain means of securing the bodies of the dead in coffins.—11th Sept.—2 months.

To Henry Constantine Jennings, of Devonshire-street, in the parish of St. Mary-le-bone, Middlesex, esq., for an instrument to be affixed to the saddle-tree, by the application and use of which, inconvenience and distress to the horse may be avoided.—11th Sept.—6 months.

To James Sprigg the elder, of Birmingham, Warwickshire, fender-maker, for a certain improvement in the manufacture of grates, fenders, and fire-iron rests.—11th Sept.—2 months.

To Thomas Wickham, of Nottingham, lace-manufacturer, for his improved and prepared rice rendered applicable for use in all cases in which starch is applied.—11th Sept.—6 months.

To William Hase, of Saxthorpe, Norfolk, iron-founder, for his new method of constructing mills or machines chiefly applicable to prison discipline.—11th Sept.—2 months.

METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr.* BURNEY *at Gosport; Mr.* CARY *in London, and Mr.* VEALL *at Boston.*

Days of Month, 1823.	Gosport, at half-past Eight o'Clock, A.M. Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Clouds. Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Height of Barometer, in Inches, &c. Lond. 1 P.M.	Bost. 8½ A.M.	Thermometer. London. 8 A.M.	Noon.	11 P.M.	Boston. 8½ A.M.	Rain. London.	Boston.	Weather. London.	Boston.
August 26	29·98	65	52½	78	N.	...	0·040	...	...	1	...	...	1	1	30·06	29·65	63	66	62	61	...	...	Rain	Cloudy—rain *p.m.*
27	30·13	68	...	70	S.	...	......	1	1	1	...	...	...	...	30·20	29·75	63	74	63	61	...	·16	Fair	Cloudy
28	30·23	64	...	62	W.	0·20	......	1	...	1	...	1	...	...	30·30	29·78	64	73	59	62·5	...	...	Fair	Fine
☾ 29	30·08	65	...	59	SE.	...	......	1	1	1	...	...	...	...	30·10	29·63	60	74	66	62	...	...	Fair	Cloudy
30	30·01	64	...	62	W.	...	·180	1	...	1	...	...	...	1	30·20	29·62	60	67	58	58	...	...	Rain	Fine
31	30·20	57	...	71	NW.	·25	......	1	1	1	...	1	...	...	30·29	29·80	58	69	58	56	...	...	Fair	Fine
Sept. 1	30·29	60	52½	78	SW.	...	......	1	1	1	...	...	...	...	30·27	29·80	58	71	60	61	...	...	Fair	Fine, rain *p.m.*
2	30·14	63	...	58	SW.	...	......	1	1	1	...	1	...	...	30·16	29·65	58	71	62	63	...	·02	Fair	Fine, rain *p.m.* wi. rainb.
3	30·14	60	...	60	NW.	·20	......	1	...	1	...	1	...	...	30·14	29·65	54	65	60	56	...	·02	Cloudy	Fine
● 4	30·28	62	...	56	W.	...	......	1	1	...	...	...	...	...	30·25	29·75	61	75	58	61·5	...	...	Fair	Fine
5	30·23	62	...	59	W.	...	......	1	...	1	...	1	...	...	30·17	29·67	60	74	60	62·5	...	...	Fair	Fine
6	30·23	62	...	55	N.	·20	......	1	...	1	...	1	...	...	30·25	29·84	57	66	55	54	...	...	Fair	Fine, rain at night
7	30·20	57	53	54	NE.	...	......	1	1	1	...	1	1	...	30·26	29·90	50	64	50	55	...	·02	Fair	Fine
8	30·27	53	...	53	NE.	...	......	1	...	...	...	1	...	...	30·35	30·01	50	60	47	54	...	...	Fair	Fine
9	30·23	51	...	51	NE.	·50	......	...	...	...	...	1	...	...	30·27	29·98	47	62	51	50	...	...	Fair	Fine
10	30·25	56	...	53	NE.	...	......	1	...	...	...	...	...	...	30·29	29·98	45	65	55	50	...	...	Fair	Fine
11	30·28	60	...	64	E.	...	......	1	1	...	...	1	...	...	30·38	30	55	69	56	60	...	...	Fair	Fine
☽ 12	30·16	60	...	58	E.	·30	......	1	...	1	...	...	...	...	30·12	29·87	55	71	60	59·5	...	...	Fair	Fine
13	29·95	65	...	57	SW.	...	......	1	1	1	...	...	...	...	29·95	29·55	66	75	61	60	...	...	Fair	Cloudy, rain at nt.
14	29·82	65	53¼	60	S.	...	......	1	1	1	...	...	1	...	29·82	29·35	60	71	67	62	...	·03	Fair [fr.	Cloudy
15	29·40	62	...	55	W.	·35	......	1	...	...	...	1	1	1	29·65	28·81	60	67	63	62·5	...	·06	thun. *a.m.*	Do. rain & lightn. *a.m.*
16	29·87	60	...	56	W.	...	·050	1	...	1	...	1	1	1	29·98	29·45	55	67	52	54·5	...	...	Fair	Fine
17	29·94	62	...	73	SW.	...	·240	1	...	1	...	...	...	1	30·01	29·50	56	67	50	58·5	...	·18	Rain	Rain
18	30·34	52	...	68	N.	·20	......	1	...	1	...	1	...	...	30·45	30·03	47	62	52	53·5	...	...	Fair	Fine
19	30·32	54	...	61	N.	...	......	1	1	1	...	1	1	...	30·33	30	46	66	56	51·5	...	...	Fair	Fine
○ 20	30·18	54	...	58	N.	...	......	1	1	1	...	1	...	...	30·20	29·85	55	62	56	50	...	...	Fair	Cloudy
21	30·00	58	53½	55	SW.	·20	·150	1	1	1	...	1	1	1	29·90	29·55	54	63	59	53	...	·22	Rain	Rain
22	29·42	55	...	61	W.	...	·040	1	...	1	...	1	1	1	29·38	29·05	52	55	50	53·5	...	8	Stormy	Cloudy—rain at nt.
23	29·96	56	...	57	NW.	...	......	...	1	1	...	1	1	1	30·02	29·66	46	56	56	48	...	·17	Cloudy	Fine—rain at night
24	29·93	61	...	71	NW.	...	......	1	1	1	...	...	...	...	29·99	29·50	57	68	60	58	...	...	Fair	Cloudy
25	30·04	60	53½	65	W.	·30	·030	...	...	1	...	...	1	1	30·10	29·63	59	63	60	61	...	...	Cloudy	Fine

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

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XLIX. *Complete Description of Erlan, a Mineral long misunderstood, and newly determined.* By AUGUSTUS BREITHAUPT, *and* C. G. GMELIN*.

I. *Determination of Erlan as a Mineral Species.* By Aug. Breithaupt, of Freiberg, Inspector of Precious Stones.

A. *Characteristic.*

ERLAN.—THIS mineral varies in lustre from glistening to dull; in the streak it has a resinous lustre: its colour is greenish-gray, usually light, the streak is white. It occurs massive; and in small and fine-granular distinct concretions, from which it passes to compact. Its fracture varies from foliated to splintery and even. Hardness from 6·25 to 7.† Specific gravity from 3·0 to 3·1.

B. *Observations respecting the History and Discovery of Erlan.*

I first saw this mineral in the autumn of 1818, lying as a flux at the smelting-furnace (Hüttenhose) of the Erla Iron-Works (commonly called Erlhammer) near Schwarzenberg, in the Saxon Erzgebirge; I was then convinced that it was not limestone, for it was much too angular, too hard, and too heavy. It has been made use of as a flux in these extensive iron-works for above two centuries, as well as in some neighbouring works; but although they have often been visited by mineralogists and chemists, yet no one ever doubted but that it was limestone. I immediately sought for the place where it was found, and ascertained that erlan mixed with mica constituted part of the oldest gneiss-formation. In one place there were also

* Abstracted from *Schweigger* and *Meinecke's Neues Journal für Chemie und Physik, N. R. band* 7, *p.* 76, where it is given as an extract from Breithaupt's Complete Characteristic of the Mineral Kingdom, a work nearly ready for publication when the extract appeared, which was in February last: the Editors inform us that this work may be considered as a supplement to Hoffmann and Breithaupt's Manual of Mineralogy.

† That is, it varies from being somewhat harder than apatite, to the hardness of sodalite or actynolite.

strata of a red felspar, almost as small and fine-granular as erlan usually is, but it may be immediately distinguished from that substance by its inferior specific gravity, which is 2·6. The mountain consisting of erlan, and a small quantity of mica, which are also mixed with slate, and which aggregate I call Erlan-rock, constitutes a portion of at least 100 fathoms in width, in the chain of the Erzgebirge, that separates the Pöhle from the Schwarzwasser. The stratification is here intersected by small parallel veins of prehnite, associated with fluor-spar, blackish-green radiated hornblende, green augite (sahlite), green epidote, copper-pyrites, copper-green, &c. The prehnite of this place, of a greenish-white colour, and partly crystallized in the well-known tabular prisms of 103°, was taken for quartz: it is remarkable that here, as almost every where else, it is accompanied with copper ore. In the preceding year erlan was found at the Teufelstein, below Schwarzenberg, but only in a compact state. I have been assured that it is found in the Flössegruben near Breitenbrunn.

The name *Erlan* refers to the place where it was first found, near the village and forge of Erla, and it may be taken as a temporary one, until the crystalline nature of the mineral be studied. I doubt not (as it is crystallized) but that some coarse-granular erlan may be found, which will show the direction of the cleavage better than the varieties hitherto discovered. All doubts respecting its identity as a mineral species would then be dissipated. I know no mineral, however, which can be easily mistaken for erlan. It most resembles gehlenite in oryctognostic characters; it is soon distinguished from felspar by its greater weight, and from saussurite (or dyskolite) by its inferior weight and hardness.

I heard that this flux is roasted before it is used; and that, for the smelting of iron ore, at Erla they mix it with an equal quantity of white granular limestone.

My highly esteemed friend Professor C. G. Gmelin, at my request, was so kind as to subject erlan to a minute chemical examination.

Chemical Examination of Erlan; by Professor C. G. Gmelin, of Tübingen.

A.

The specific gravity of the purest foliated erlan, determined at the temperature of 54·5 Fahrenheit, was 1·7507.* The specimen employed weighed 23 grammes.

* There must be an error here, perhaps in writing only, as the specific gravity of erlan is always between 3·0 and 3·1.—*Breithaupt.*

B. Before

B.

Before the blowpipe, without addition, it melts into a transparent bead, free from bubbles, and but slightly coloured. With borax it becomes a transparent greenish glass. Phosphoric salt decomposes it, leaving a skeleton of silica; but the bead, when cool, remains transparent: if more of the pulverized stone be added, the bead, partly transparent while in fusion, becomes quite opake when cool. Soda, in small quantities, melts with the pulverized stone, but in large quantities it does not possess this property.

C.

a.) 4·925 grammes of the pulverized mineral dried by a spirit-lamp, left, after having been strongly heated, 4·899 gr.: 100 parts, therefore, contain 0·606 of volatile matter.

b.) 5 gr. dried by the spirit-lamp were heated in a platinum crucible, for two hours, with 25 gr. of carbonate of barytes. The fused mass was in one piece, of a grey-yellow colour; it was dissolved in water, decomposed by muriatic acid, and evaporated. The silica after having been heated weighed 2·658 gr. or 53·160 *per cent.*

c.) The solution, freed from barytes by sulphuric acid, was afterwards evaporated nearly to dryness: some sulphate of lime became separated from it, which, after having been washed on a filter, dried, and heated, weighed 1·495 gr. containing 0·62087 of lime, or 12·417 *per cent.*

d.) The liquid separated from the sulphate of lime, gave, by means of caustic ammonia, a precipitate, which was heated with excess of caustic potassa, and alumina was obtained from the alkaline solution by saturation with muriatic acid, and precipitation by carbonate of ammonia: it weighed, when dried, 0·7017, or 14·034 *per cent.*

e.) The brown residuum which remained after separating the alumina by caustic potassa weighed 0·3718. By means of succinic acid, and precipitation with a boiling solution of carbonate of soda, it was decomposed into 0·3569 of oxide of iron, =7·138 *per cent.*, and 0·01491 of oxide of manganese, =0·299 *per cent.*

f.) From the fluid mixed with caustic ammonia that had passed the filter, oxalate of ammonia precipitated oxalate of lime, which gave 0·17557 of carbonate of lime, containing 0·09902 of lime, =1·980 *per cent.*

g.) The solution, entirely freed from lime, was now evaporated and heated. An unmelted mass remained, which indicated a large proportion of magnesia. It was dissolved in water, and mixed with hydrosulphuret of ammonia, the precipitate decomposed by muriatic acid; and the acid fluid, in a boiling

 state,

state, was precipitated by carbonate of soda: 0·017 gr. of oxide of manganese was obtained, =0·340 *per cent.*

h.) The excess of hydrosulphuret of ammonia being removed, the liquid was decomposed by acetate of barytes; the sulphate of barytes was separated by filtration, and the fluid containing acetic acid being evaporated, and heated, and the residuum boiled in water, 0·217 gr. of carbonate of soda were obtained, containing 0·13057 of soda, =2·611 *per cent.* It must be observed, however, that in dissolving this carbonate of soda in water, some traces of magnesia remained undissolved, which had previously been dissolved in the alkaline solution.

i.) The residual matter (*h*) was dissolved in muriatic acid, the barytes precipitated by sulphuric acid, and separated on a filter; and the liquid in a state of ebullition precipitated by carbonate of soda: 0·271 gr. =5·420 *per cent.* of pure magnesia was obtained, which entirely crystallized with sulphuric acid, into sulphate of magnesia.

The fluoric and phosphoric acids were sought for, in an assay made for the purpose, but no traces of them were discovered. Erlan, therefore, consists of

Silica	53·160
Alumina	14·034
Lime	14·397
Soda	2·611
Magnesia	5·420
Oxide of iron	7·138
Oxide of manganese	0·639
Volatile matter	0·606
	98·005

L. *Derivative Analysis; being a new and more comprehensive Method of the Transformation of Functions than any hitherto discovered: extending not only to the Extraction of the Roots of Equations, but also to the Reduction of Quantities from the Multiples of Powers or Products to other equivalent Expressions, by which the Summation of any rational Series may be readily effected. By Mr.* Peter Nicholson*.

5 Claremont-place, Judd-street.

To the Editors of the Philosophical Magazine and Journal.

Part 1.—*Multiplication.*

MULTIPLICATION is performed in the usual way; but instead of the compound coefficients of the entire product, substitute a letter for the amount or aggregate of each such coefficient; then as many equations as the entire product has

* Communicated by the Author.

has coefficients will be formed, which will show the relation between the succeeding and the preceding coefficients of the entire product, or between the coefficients of the entire product and those of the multiplicand and multiplier.

Ex. 1. Multiply $A+Bx+Cx^2+Dx^3+Ex^4+$&c. by the binomial $a+x$.

Operation.

$$\begin{array}{l} A + Bx + Cx^2 + Dx^3 + Ex^4 + \&c. \\ \underline{a + x \qquad\qquad\qquad\qquad\qquad\qquad} \\ aA + aBx + aCx^2 + aDx^3 + aEx^4 + \&c. \\ \underline{\qquad\quad Ax + Bx^2 + Cx^3 + Dx^4 + \&c.} \\ A_1 + B_1x + C_1x^2 + D_1x^3 + E_1x^4 + \&c. \end{array}$$

From which we have the following derivative equations, viz.

$$\begin{aligned} A_1 &= \quad aA \\ B_1 &= A + aB \\ C_1 &= B + aC \\ D_1 &= C + aD \end{aligned}$$

Hence it appears that the entire product may be derived from the multiplicand, and the constant part of the multiplier. Since any coefficient of the entire product is equal to the partial product of the corresponding coefficient of the multiplicand, and the constant part of the multiplier plus the preceding coefficient of the multiplicand.

Ex. 2. Multiply $x^{n-1}+Bx^{n-2}+Cx^{n-3}+Dx^{n-4}+Ex^{n-5}+$&c. by the binomial $x+a$.

$$\begin{array}{l} x^{n-1} + Bx^{n-2} + Cx^{n-3} + Dx^{n-4} + Ex^{n-5} + \&c. \\ \underline{x + a \qquad\qquad\qquad\qquad\qquad\qquad\qquad} \\ x^n + Bx^{n-1} + Cx^{n-2} + Dx^{n-3} + Ex^{n-4} + \&c. \\ \underline{\quad + ax^{n-1} + aBx^{n-2} + aCx^{n-3} + aDx^{n-4} + \&c.} \\ x_n + B_1x^{n-1} + C_1x^{n-2} + D_1x^{n-3} + E_1x^{n-4} + \&c. \end{array}$$

Whence we have the following derivative equations, viz.

$$\begin{aligned} B_1 &= B + a \\ C_1 &= C + aB \\ D_1 &= D + aC \\ E_1 &= E + aD \\ &\&c. \end{aligned}$$

From which it appears that the entire product may be derived from the multiplicand; for the coefficient of any term of the entire product is equal to the coefficient of the corresponding term of the multiplicand plus the partial product of the preceding term of the multiplicand, and the second part of the multiplier.

Ex. 3. Multiply the series $1+ax+a^2x^2+a^3x^3+$&c. by the series $1+bx+b^2x^2+b^3x^3+$&c.

Put

Put $B=a$, $C=a^2$, $D=a^3$ &c. and the operation will be

$$\begin{array}{r}
1+Bx+Cx^2+Dx^3+\&c.\\
1+bx+b^2x^2+b^3x^3+\&c.\\
\hline
1+Bx+Cx^2+Dx^3+\&c.\\
bx+bBx^2+bCx^4+\&c.\\
+b^2x^2+b^2Bx^2+\&c.\\
+b^3x^3+\&c.\\
+\&c.\\
\hline
1+B_1x+C_1x^2+D_1x^3+\&c.
\end{array}$$

Where $B_1=B+b$,

$C_1=C+bB+b^2=C+b(B+b)=C+bB_1$

$D_1=D+bC+b^2B+b^3=D+b(C+bB+b^2)=D+bC_1$

&c. &c.

In the same manner by taking the series $1+B_1x^2+C_1x^2+D_1x^3+$&c. as a multiplicand and the series $1+cx+c^2x^2+c^3x^3+$&c. as a multiplier; then if the entire product be $1+B_2x+C_2x^2+D_2x^3+$&c. we shall have by the same law

$B_2=B_1+C$

$C_2=C_1+cB_1+c^2=C_1+cB_2$

$D_2=D_1+cC_1+c^2B_1+c^3=D_1+c(C_1+cB_1+c^2)=D_1+cC_2$

&c. &c.

and so on for the product of any number of series; therefore, arranging these values according to the number of products, there will arise

$$\begin{array}{l|l|l|l}
B_1=B+b & C_1=C+bB_1 & D_1=D+bC_1 & \\
B_2=B_1+c & C_2=C_1+cB_2 & D_2=D_1+cC_2 & \\
B_3=B_2+d & C_3=C_2+dB_3 & D_3=D_2+dC_3 & \&c.\\
\&c. & \&c. & \&c. &
\end{array}$$

Let it be required to find all the combinations of the letters a, b, c, equally with one another to the third order.

Now observing that $B=a$, $C=a^2$, $D=a^3$, then will

$B_1=a+b$

1st order. $B_2=$ ——— $+c$

$C_1=a^2+ab+b^2$

2d order. $C_2=$ ———————— $+ac+bc+c^2$

$D_1=a^3+a^2b+ab^2+b^3$

3d order. $D_2=$ ——————————— $+a^2c+abc+b^2c+ac^2+bc^2+c^3$

&c.

Where the long line stands for all the combinations of the next line above it.

Again, let it be required to find all the orders of the combinations of the letters aaa, bb, c, or a^3, b^2, c, or let all the divisors of 360 be required; now $360=2^3.3^2.5=a^3b^2c$.

Here $B_1=a+b$

1st order. $B_2=$ ——— $+c$

$C_1=a^2+ab+b^2$

2d order. $C_2=$ ———————— $+ac+bc$ for c^2 is not wanted

$$
\begin{array}{ll}
 & D_1 = a^3 + a^2b + ab^2 \text{ for } b^3 \text{ is not wanted} \\
\text{3d order.} & D_2 = \text{———} + a^2c + abc + b^2c \\
\hline
 & E_1 = a^3b + a^2b^2 \text{ no higher than } a^3 \text{ or } b^2 \text{ being wanted} \\
\text{4th order.} & E_2 = \text{———} + a^3bc + a^2b^2c \\
\hline
 & F_1 = a^3b^2 \\
\text{5th order.} & F_2 = \text{———} + a^3bc + a^2b^2c \\
\hline
 & G_1 = o \\
\text{6th order.} & G_2 = a^3b^2c
\end{array}
$$

Part II.—*Division.*

Division is performed in the usual way, viz. by arranging the parts of the dividend in a line according to the natural or inverse order of the powers of the variable, and the parts of the divisor in the same order.

Divide the first part of the dividend by the first part of the divisor, and the result is the first part of the quotient.

Multiply the first part of the quotient successively, by every part of the divisor, and place the products so that the powers of the variable may be under the same powers in the dividend.

Draw a line underneath and write in a line below the line thus drawn the same powers of the variable as those immediately above, except in the first place, and prefix a new letter to each power as a coefficient which will form the first remainder.

Annex the next part of the dividend from which no subtraction has been made to this remainder, and consider this remainder so increased as a second dividend; then proceed to find the second or next part of the quotient, and the third or next dividend as before; and so on, as far as may be necessary. In any convenient place write the letters thus substituted, and their values, in the form of equations; that is, every letter equal to the aggregate of the two coefficients of the corresponding power above, considering the sign of the lower of these two changed by subtraction.

Then the table thus formed will show the law of derivation by which the real quotient may be obtained.

Ex. 1. Divide $\alpha + \beta x + \gamma x^2 + \delta x^3 + \&c.$ by $1 - bx$ put $A = \alpha$; then proceed with the operation,

$$
\begin{array}{l|l}
\text{Dividend.} & \text{Divisor.} \\
A + \beta x + \gamma x^2 + \delta x^3 + \&c. & 1 - bx \\
A - Abx & \text{quotient} \\
\hline
\quad Bx + \gamma x^2 & A + Bx + Cx^2 + \&c. \\
\quad Bx - Bbx^2 & \\
\hline
\qquad Cx^2 + \delta x^3 & \\
\qquad Cx^3 - Cbx^3 & \\
\hline
\qquad\quad Dx^3 + \&c. & \\
\qquad\quad \&c. &
\end{array}
$$

By

By this operation we have the following derivative table, viz.

$$\begin{aligned} A &= \alpha \\ B &= Ab+\beta \\ C &= Bb+\gamma \\ D &= Cb+\delta \\ &\text{\&c.} \end{aligned}$$

From which it appears that the nth coefficient of the quotient is equal to the product of the next preceding coefficient, and the coefficient of the second term of the divisor plus the nth coefficient of the dividend. Whence by the table we derive the coefficients of the quotient thus,

$$\begin{aligned} A &= & \alpha &= \alpha \\ B &= Ab+\beta &&= \alpha b+\beta \\ C &= Bb+\gamma &&= \alpha b^2+\beta b+\gamma \\ D &= Cb+\delta &&= \alpha b^3+\beta b^2+\gamma b+\delta \\ &\text{\&c.} && \text{\&c.} \end{aligned}$$

Whence

$$A+Bx+Cx^2+\text{\&c.}=\alpha+(\alpha b+\beta)x+(\alpha b^2+\beta b+\gamma)x^2+\text{\&c.}$$

Ex. 2. Divide the infinite series $\alpha+\beta x+\gamma x^2+\delta x^3+\varepsilon x^4+\text{\&c.}$ by $a-bx-cx^2$.

Operation.

Dividend. | Divisor.

$$\alpha+\beta x+\gamma x^2+\delta x^3+\varepsilon x^4+\text{\&c.} \quad \Big| \quad a-bx-cx^2$$

Quotient

$$\frac{\alpha}{a}+\frac{B}{a}x+\frac{C}{a}x^2+\text{\&c.}$$

$$\begin{array}{l} \alpha-\frac{b\alpha}{a}x-\frac{c\alpha}{a}x^2 \\ \hline Bx+B_1x^2+\delta x^3 \\ Bx-\frac{bB}{a}x^2-\frac{cB}{a}x^3 \\ \hline Cx^2+C_1x^3+\varepsilon x^4 \\ Cx^2-\frac{bC}{a}x^3-\frac{cC}{a}x^4\text{\&c.} \\ \hline Dx^3+D_1x^4+\text{\&c.} \\ \text{\&c.} \qquad \text{\&c.} \end{array}$$

From which operation we have the following derivative table, viz.

$$B=\frac{\alpha b+\beta a}{a} \qquad B_1=\frac{\alpha c+\gamma a}{a}$$

$$C=\frac{Bb+B_1a}{a} \qquad C_1=\frac{Bc+\delta a}{a}$$

$$D=\frac{Cb+C_1a}{a}$$

&c. &c.

From this table we derive the real quotient

$$\frac{\alpha}{a}+\frac{\alpha b+\beta a}{a^2}x+\frac{\alpha b^2+\beta ab+\alpha ac+\gamma a^2}{a^3}x^2+\text{\&c.}$$

But

But if the first part a of the divisor were unity, the derivative table would be simply

$$\begin{array}{l|l|l|l|l}
B = \alpha b + \beta & & & & \\
B_1 = \alpha c + \gamma & C = Bb + B_1 & & & \\
 & C_1 = Bc + \delta & D = Cb + C_1 & & \\
 & & D_1 = Cc + \varepsilon' & E = Db + D_1 & \\
 & & & \&c. & \&c.
\end{array}$$

And the quotient derived from this table would be simply

$$\alpha + Bx + Cx^2 + \&c. =$$
$$\alpha + (\alpha b + \beta)x + (\alpha b^2 + \beta b + \alpha c + \gamma)x^2 + \&c.$$

Ex. 3. Divide the series $\alpha + \beta x + \gamma x^2 + \delta x^3 + \varepsilon x^4 + \&c.$ by the series $1 - bx - cx^2 - dx^3 - ex^4 - \&c.$

Operation.

$$\begin{array}{r|l}
\text{Dividend.} & \text{Divisor.} \\
 & 1 - bx - cx^2 - dx^3 - ex^4 - \&c. \\
\alpha + \beta x + \gamma x^2 + \delta x^3 + \varepsilon x^4 + \&c. & \text{Quotient} \\
\alpha - \alpha bx - \alpha cx^2 - \alpha dx^3 - \alpha ex^4 - \&c. & \alpha + Bx + Cx^2 + Dx^3 + \&c. \\
\hline
Bx + B_1x^2 + B_2x^3 + B_3x^4 + \&c. & \\
Bx - bBx^2 - cBx^3 - dBx^4 - \&c. & \\
\hline
Cx^2 + C_1x^3 + C_2x^4 + \&c. & \\
Cx^2 - bCx^3 - cCx^4 - \&c. & \\
\hline
Dx^3 + D_1x^4 + \&c. & \\
Dx^3 + bDx^4 - \&c. & \\
\hline
Ex^4 + \&c. & \\
Ex^4 - \&c. &
\end{array}$$

By this operation we have the following derivative equations, viz.

$$\begin{array}{l|l|l|l|l}
B = b\alpha + \beta & & & & \\
B_1 = c\alpha + \gamma & C = bB + B_1 & & & \\
B_2 = d\alpha + \delta & C_1 = cB + B_2 & D = bC + C_1 & & \\
B_3 = e\alpha + \varepsilon & C_2 = dB + B_3 & D_1 = cC + C_2 & E = bD + D_1 & \\
\&c. & \&c. & \&c. & \&c. & \&c.
\end{array}$$

By multiplying and adding as this table directs, we shall have the real coefficients of the powers of x in the quotient, viz.

$$\begin{aligned}
A &= \alpha \\
B &= \alpha b + \beta \\
C &= \alpha b^2 + \beta b + \alpha c + \gamma \\
D &= \alpha b^3 + \beta b^2 + \alpha cb + \gamma b + \alpha bc + \beta c + \alpha d + \delta \\
&\qquad \&c.
\end{aligned}$$

Or, if the divisor had been $a - bx - cx^2 - dx^3 - \&c.$ instead of $1 - bx - cx^2 - dx^3 - \&c.$ and if A had been the first term of

 the

the quotient, B, C, D, &c. the coefficients of the following terms, we should have had

$$A = \frac{\alpha}{a}$$

$$B = \frac{\alpha b + \beta a}{a^2}$$

$$C = \frac{\alpha b^2 + \beta a b + \alpha a c + \gamma a^2}{a^3}$$

$$D = \frac{\alpha b^3 + \beta a b^2 + 2\alpha a b c + \gamma a^2 b + \beta a^2 c + \alpha a^2 d + \delta a^3}{a^4}$$

&c. &c.

That is, by making up the sum of the indices to the same number as the highest in each part of each numerator, and making the denominators respectively a, a^2, a^3, a^4, &c., so that if we have the coefficients in one way, we can easily find them in the other.

But if α, β, γ, &c., and a, b, c, &c. had been given in numbers, the values of A, B, C, the coefficients of the quotient, would have been found much more easily by the rule directed in the table, as we shall have occasion to show hereafter.

Ex. 4. Divide the series $\alpha x^m + \beta x^{m-1} + \gamma x^{m-2} + \delta x^{m-3} + \&c.$ by the series $x^n - bx^{n-1} - cx^{n-2} - dx^{n-3} - \&c.$

Divisor.

$$\begin{array}{r|l}
 & x - bx^{n-1} - cx^{n-2} - dx^{n-3} - \&c. \\
\alpha x^m + \beta x^{m-1} + \gamma x^{m-2} + \delta x^{m-3} + \&c. & \text{Quotient.} \\
\underline{\alpha x^m - b\alpha x^{m-1} - c\alpha x^{m-2} - d\alpha x^{m-3} - \&c.} & \alpha x^{m-n} + Bx^{m-n-1} + Cx^{m-n-2} + \&c. \\
Bx^{m-1} + B_1 x^{m-2} + B_2 x^{m-3} + \&c. & \\
\underline{Bx^{m-1} - bBx^{m-2} - cBx^{m-3} - \&c.} & \\
Cx^{m-2} + C_1 x^{m-3} + \&c. & \\
\underline{Cx^{m-2} - bCx^{m-3} - \&c.} & \\
Dx^{m-3} + \&c. & \\
\underline{Dx^{m-3} - \&c.} & \\
\&c. &
\end{array}$$

By this operation we have the following derivative equations,

$$\begin{array}{l|l|l|l|l}
B = b\alpha + \beta & & & & \\
B_1 = c\alpha + \gamma & C = bB + B_1 & & & \\
B_2 = d\alpha + \delta & C_1 = cB + B_2 & D = bC + C_1 & & \\
B_3 = e\alpha + \varepsilon & C_2 = dB + B_3 & {}_1D = cC + C_2 & E = bD + D_1 & \\
\&c. & \&c. & \&c. & \&c. & \&c.
\end{array}$$

which are the same as those in the table of the preceding example.

Ex. 5.

Ex. 5. Divide the quantity A by the binomial $v-e$.

$$
\begin{array}{l|l}
 & \text{Divisor.} \\
A & v-e \\
A-\dfrac{Ae}{v} & \text{Quotient} \\
\hline
\qquad \dfrac{B_1}{v} & \dfrac{A}{v}+\dfrac{B_1}{v^2}+\dfrac{C_1}{v^3}+\dfrac{D_1}{v^4}+\&c. \\
\qquad \dfrac{B_1}{v}-\dfrac{eB_1}{v^2} & \\
\hline
\qquad\qquad \dfrac{C_1}{v^2} & \\
\qquad\qquad \dfrac{C_1}{v^2}-\dfrac{eC_1}{v^3} & \\
\hline
\qquad\qquad\qquad \dfrac{D_1}{v^3}-\&c. & \\
\qquad\qquad\qquad \&c. &
\end{array}
$$

By this operation we have the following derivative equations, viz.

$$
\begin{aligned}
B_1 &= eA \\
C_1 &= eB_1 \\
D_1 &= eC_1 \\
&\&c.
\end{aligned}
$$

Hence it appears that any quotient figure is equal to the product of the next preceding quotient figure by the second part of the divisor.

Ex. 6. Divide the series $B+\frac{A}{v}+\frac{B_1}{v^2}+\frac{C_1}{v^3}+\frac{D_1}{v^4}+\&c.$ (which is the quotient in the preceding example increased by the quantity B) by the binomial $v-e$ (which is the same divisor as in the preceding example.

$$
\begin{array}{l|l}
 & \text{Divisor.} \\
B+\dfrac{A}{v}+\dfrac{B_1}{v^2}+\dfrac{C_1}{v^3}+\dfrac{D_1}{v^4}+\&c. & v-e \\
 & \text{Quotient} \\
B-\dfrac{eB}{v} & \dfrac{B}{v}+\dfrac{B_2}{v^2}+\dfrac{C_2}{v^3}+\dfrac{D_2}{v^4}+\&c. \\
\hline
\qquad \dfrac{B_2}{v}+\dfrac{B_1}{v^2} & \\
\qquad \dfrac{B_2}{v}+\dfrac{eB_1}{v^2} & \\
\hline
\qquad\qquad \dfrac{C_2}{v^2}+\dfrac{C_1}{v^3} & \\
\qquad\qquad \dfrac{C_2}{v^2}-\dfrac{eC_2}{v^3} & \\
\hline
\qquad\qquad\qquad \dfrac{D_2}{v^3}+\dfrac{D_1}{v^4} &
\end{array}
$$

By

By this operation we have the following derivative equations,

$$B_2 = eB + A$$
$$C_2 = eB_2 + B_1$$
$$D_2 = eC_2 + C_1$$
$$\&c.$$

which show the law of derivation.

[To be continued.]

LI. *Discovery of the secret Destroyers of the Trees in St. James's Park.*

To the Editors of the Philosophical Magazine and Journal.

THE alterations which are now taking place in the Parks, and particularly in St. James's Park, evidently excite a good deal of public interest. Few persons, however, seem aware that the greatest change which is about to be effected in the appearance of these "*Lungs of London*," is one which is contemplated by none with less pleasure than by those who have the care of them. Few persons suspect, for instance, that in a very short period St. James's Park will be clothed in the dapper dress of a nursery plantation, and will have lost those shady avenues and that antiquated appearance which are all associated with the recollections of times gone past. So rapidly, however, is it advancing to this state, that every person who is in the habit of entering it must perceive that, unless some remedy be quickly applied, a few months have only to elapse when there will be scarcely any thing green in it but the grass. Of the saplings which have been lately planted I do not speak; but it is manifest that every thing deserving the name of a tree, a few limes only excepted, is rapidly disappearing. In spring we see the leaves sprout forth from the venerable trunks in all the luxuriance of vegetation, when of a sudden they are blasted as if by lightning, the bark falls from the stem, and long ere winter the finest tree perhaps in the park is only fit for fire-wood. Whole rows have thus disappeared and are still rapidly disappearing in the Mall and Bird-Cage Walk; and as it is anticipated that the public will esteem this open condition of the park to be little conducive to its beauty, even if it should add to its salubrity, great pains have of course been taken to find out the cause of the mischief.

As it was clear that the trees died in consequence of being completely stripped of their bark, rewards were at first offered for the discovery of the persons who so mischievously barked them; but in vain. It was observed, however, very ingeniously, that

that no more of the tree was barked from the ground than what was easily within the reach of a soldier's bayonet; and this was sufficient to throw suspicion on some unfortunate recruits, of whom more than one was arrested without producing any diminution of the evil. In vain were persons employed to sit up during whole nights watching for the enemy; the bark continued to be found every morning on the ground at the roots of the trees, and the park-keepers, after all their trouble, could only conclude, "that the bark fell off in consequence of something being placed on the trunks during the day-time." In this conclusion we shall see that they were right; but the criminals, as well as their motives for such obstinate perseverance in downright mischief, have hitherto remained undiscovered, in spite of every offered reward and threatened punishment. As, however, I have been for some time past in possession of their names, and as they continue obstinately in their mischievous courses, I trust you will allow me the use of your pages, in order that the criminals may be held up to general reprobation.

Wilhelm, in reciting the alarm that was occasioned in Germany by the ravages made in 1783 by the *Scolytus typographus*, an insect which destroyed whole forests of pines so as to threaten the inhabitants of the Hartz with a total suspension of their mining operations, asks, Who would believe that so small a beetle can thus render itself more formidable to mankind than the strongest and most fierce beast of prey? If we however were to ask, Who would believe that the author of all the above mischief in the parks, is a small beetle of the same natural family as the *Scolytus typographus*, and scarcely 1-6th of an inch in length? there is no entomologist but would answer, that he is every day in the habit of meeting with similar wonders. Indeed, entomologists have long been aware that it is nothing else than the evil which is termed in Germany *Wurm-trökness* (decay caused by worms) which is at present devastating St. James's Park. However, they were unfortunately not believed until the disease had reached that pitch which at present seems almost to make remedy hopeless.

I verily believe that in 1819 scarcely two trees were attacked in St. James's Park. In the summer of 1820 I first noticed a tree completely barked in the Bird-Cage Walk; and the myriads of holes with which the trunk was perforated soon pointed out the cause to be entomological, which was afterwards confirmed by my taking many specimens of the *Hylesinus Destructor* out of them. If this tree had been then cut down and burnt, in all probability the progress of the disease would have been arrested; whereas now every elm is in some degree

degree infected, and every week we may observe that a tree has perished.

I have taken three different species of coleopterous insects on the elms in the Park, viz. *Hylesinus Destructor* Latr., *Scolytus ligniperda* Latr., and *Hypophlœus bicolor* Latr. As they are all *decorticators*, they no doubt all tend to produce the effects we witness; but as the great criminal is the *Hylesinus Destructor*, I shall content myself with giving his history, from which that of the others little differs.

The *Hylesinus Destructor* is a brown beetle with a polished black head and thorax, and is in this its perfect or winged state throughout the summer months. It appears to confine its attacks in a great degree to the elm, which by the way ought to prevent for the present the planting of any young elms in the park. When the insect has laid its eggs in the crevices under the bark of this species of tree, it soon dies. The larvæ, however, that are hatched from these eggs pierce their way into the wood, and remain there feeding at their ease during the winter. About the end of this season they return towards the surface of the trunk and assume the pupa state, when in spring the first symptom of the disease appears, by the crevices of the bark being full of what seems a very fine sort of saw-dust. This results from the continued attempts of the perfect *Hylesini* on leaving their pupa state to arrive at the external air. The bark indeed is soon loosened from the stem by their endeavours, and at length falls in large pieces, when the leaves turn yellow, wither, and the tree finally perishes; but not before a new brood of larvæ has been hatched to spread further devastation in a future year under the form of winged insects. It thus is evident that winter is the proper time to cut down such dead elms, which ought then to be burned with the larvæ contained in them. Hitherto however the time selected for cutting down the dead elms in the Park, has been just after all the mischief for the season has been effected, and when these nurseries of *Hylesini* have sent forth their inhabitants to the air for the benefit of such trees as might have remained free from infection. Perhaps it may yet be worth while to make the experiment, whether such trees may not be preserved sound and intact by having their trunks coated over at the proper season with some vegetable pitch.

If these remarks should be of any service towards preserving the ancient appearance of the parks, I shall be glad; but I repeat, that I fear it is too late, and that my principal satisfaction in making this communication to you must result from my having "*thrown the blame on the right shoulders.*"

I am, gentlemen, yours, &c.

DENDROPHILUS.

LII. *Remarks on the Identity of certain General Laws which have been lately observed to regulate the Natural Distribution of Insects and Fungi.* *By* W. S. MacLeay, *Esq. M.A. F.L.S.*

[Concluded from p. 200.]

IN the first place, M. Fries lays it down as a rule, which is quoted above, that he admits no groups whatever to be natural unless they form circles more or less complete. Let us then apply this rule to what he terms his central group, and which he makes always to consist of two. Does this form a circle? If not, the group cannot be natural according to his own definition.

If, on the other hand, its two component groups are each circles, then these are natural. Thus the *Ptilota* will not form one circle, but two; consequently they form two natural groups, which is furthermore proved by their parallel relations of analogy. If we turn to Fungi also, the *Hymenini*, according to M. Fries, do not form one circle, but two; one of *Pileati*, the other of *Clavati*; so that instead of the *Hymenomycetes* forming four natural groups, viz. *Sclerotiacei*, *Tremellini*, *Uterini*, and *Hymenini*, they form, if our author be correct, five; viz. *Sclerotiacei*, *Tremellini**, *Uterini*, *Pileati*, and *Clavati*.

But, to understand this still better, we had as well perhaps enter a little deeper into our author's theory. Every group, he says, which expresses well the character of the superior group to which it belongs, is called the *centrum*: by this, not meaning the centre of a circle, but the site of the normal form or perfection of the particular structure common to the superior group, of which it forms a part. The word *perfection*, even as here used, requires explanation; for it does not, as might be supposed, in this place signify affinity to any particular group. Our author, on the contrary, most properly says, that the idea of perfection in structure has nothing to do with affinity†. "Ipsa hæc affinitas imperfectionem potius indicat;

* This appears to be one of those interesting groups which connect the least perfectly organized beings with those which are the most perfectly organized. In the department of *Hysterophyta* it is to the *Coniomycetes* or lowest *Fungi*, what in the animal kingdom the *Vermes* are to the *Acrita*.

† To the general observations on this subject, as connected with the animal kingdom, which I have given in *Horæ Entomologicæ*, p. 205, I may add the botanical authority of Professor Schweigger. "Nec etiam genera et ordines plantarum in lineam a cryptogamicis ad dicotyledoneas progredientem ita disponi possunt, ut familia quævis præcedentis structuram magis evolutam præbeat. Vix ullus de vegetabilium serie usitata, a cotyledonum numero deducta, affirmat, plantas dicotyledoneas omni ratione monocotyledoneis

indicat; perfectissima enim sunt in quâvis sectione ab omnibus aliis remotissima. Sic perfectissima animalia et vegetabilia, quæ maxime a se invicem remota; infima, quorum limites confluunt." Hence it follows, that the *centrum*, or perfection of a group, is in fact that part of the circumference of the circle of affinity which is furthest from the neighbouring group, and exactly the same thing with what in the *Horæ Entomologicæ* has perhaps more happily been called *Type*.

Indeed the confusion arising from the use of the word *centrum*, as applied to a point in the circumference of a circle, is still increased by applying the word *radii* to those groups likewise in the circumference which lead from one centrum or type to another, and which I have termed *annectent groups**. The use of these terms *centrum* and *radii* is the more unfortunate, as our author never for a moment takes them in any other sense than that in which I have used the expressions *type* and *annectent groups*. When, therefore, he says that in every group, whether class, order, &c. there are a *centrum* and *radii*, we must understand him as meaning, that there are in every circle first a *type* or normal form expressing the perfection of the superior group to which it belongs; and secondly, *annectent groups* connecting this type with other groups. Or, to take his own words, "In *centrum* quod species plurimas continet, character optime quadrat. *Radii* ad reliquas classes (scilicet ordines, genera, &c.) abeuntes, utriusque classis characterem conciliant, sed ad illam (viz. the typical group) cujus character maxime eminet referuntur."

If then the determinate number in which *Fungi* are naturally grouped be four, and if it thus appears that, according to M. Fries, every natural group is a circle, having in its circumference a point of perfection or typical group called a *centrum*, and annectent groups called *radii*, it is evident that there must be one centrum and three radii for every group. But observe what immediately follows as the result of M. Fries's observation: "*Centrum* abit semper in duas series, inferiorem et superiorem, quarum illa ad antecedentem hæc ad sequentem classem (l. radium) evidentius accedit."

This rule being determined, M. Fries goes on moreover to say, that these two series which compose the *centrum* are always

ledoneis esse anteponendas." p. 6. *De Plantarum Classificatione Naturali Disquisitionibus Anatomicis et Physiologicis stabilienda Commentatio, Auctore A. F. Schweigger, &c. Regiomonti* 1820.

* There are several other terms used by M. Fries to designate his groups, and which differ from those employed by me to express the nature of similar groups. Thus, his *intermediate genera* are my *osculant genera;* his *subordinate genera* are my *types of form* or *sub-genera*, &c.

ways analogous at their corresponding points. Consequently, in every circle he admits the existence of two central groups and three radial; that is, in all, Five natural groups. Now this truly is the case throughout the whole animal kingdom. Organized Matter is the *centrum* of matter, and is composed of animals and vegetables. *Articulata**, or animals possessing an articulated axis, form the centrum of the animal kingdom, and are composed of *Vertebrata* and *Annulosa.* The *Ptilota* of Aristotle, or winged insects, form the centrum of the *Annulosa*, and are divided into *Mandibulata* and *Haustellata.* And so on, we shall ever find a natural group to be a circle of five minor groups, and that two of these minor groups form what M. Fries would call a *centrum*, or, more correctly, have some character in common which distinguishes them from the other three. That neither of these groups, viz. Organized Matter, *Articulata*, or *Ptilota*, is a circle, must be obvious to every observer; and consequently they do not fall within the sphere of M. Fries's definition already given of a natural group, but each of them forms two circles, which therefore, according to our author, are natural groups. We might turn even to the well-known great division of the vegetable kingdom into phænogamous or cotyledonous and cryptogamous or acotyledonous plants, where the former are clearly the *centrum*, and divisible into two natural groups; but surely enough has been said to show, that the notion of M. Fries on this head is in every respect, but the mode of expressing it, the same identically with mine. When he states the determinate number to be four, and we investigate the signification attached by him to this proposition, we discover that it is in effect five. How M. Fries was led to the number four, we have already endeavoured to explain; and it is truly worthy of observation, as an almost conclusive argument for the determinate number being five, that M. Fries himself is at last obliged to adopt it. This open abandonment of his theoretical number *four*, which we have seen that he had virtually abandoned before, takes place moreover in that part of his work which, relating to the more minute groups, is therefore most independent of theory, and most subjected to the keenness of practical observers. Here, in brief, he finds himself tied down to stubborn facts, and it is rather interesting to mark the result. The only genera of *Hymenomycetes Pileati* which he discovers to be divisible are, *Agaricus*, *Cantharellus*, *Thelephora*. *Hydnum*, *Boletus*, *Polyporus* and *Dædalea*, some of which, as *Agaricus*, are, as he says,

* This name has been applied to the *Annulosa*, as characterizing them alone, but improperly, inasmuch as the vertebrated animals are articulated.

says, of the first dignity; others, as *Cantharellus*, of the second*. Now every one of these genera, or at least their typical groups, are divided by M. Fries himself into five, with the single exception of *Cantharellus*; and so truly natural or dependent upon relations of analogy are these five subdivisions, that he proposes to make use of one set of names for all, and in fact does in general make use of the same name for analogous groups†. Nay more: when he has divided the well-known genus *Agaricus* into *five* natural series, he observes, "Singula series a naturâ fixè determinata clausa est reliquis parallela. Tribus diversarum serierum analogas diu eodem nomine salutavi." So that *Agaricus* is, according to the confession of M. Fries, formed of five natural series each closed up; in other words, each a circle, and corresponding at their parallel points to such a degree, that he declares it possible to assign the same names to the analogous groups.

It were tedious to proceed much further on this subject; and therefore, without entering into the speculations, often unintelligible and always vague, of Plutarch, Sir Thomas Brown, Drebel, Linnæus and others, as to the doctrine of *quintessence* generally, we may at once set forth the last argument which shall now be produced for the existence of a quinary distribution in organized nature. It may be stated thus: In the year 1817 I detected a quinary arrangement‡ in considering a small portion of coleopterous insects; and in the year 1821 I attempted to show that it prevailed generally throughout nature. In the same year (1821), and apparently without any view beyond the particular case then before him, M. Decandolle stated the natural distribution of Cruciferous plants to be quinary. And again, in the same year, a third naturalist, without the knowledge of either Decandolle's *Mémoire* or the *Horæ Entomologicæ*, and in a different part of Europe, publishes what he considers to be the natural arrangement of *Fungi*. Arguing *à priori*, this third naturalist fancies that the determinate number into which these acotyledonous plants are distributed ought to be four; but finds it necessary, in order that it may coincide with observed facts, to make it virtually five. Nay, at last, in spite of the prejudice of theory, he is unable to withstand the force of truth, throws himself into the arms of Nature, and declares that where he actually finds his

* The groups here said to be of the second dignity, appear to be of the same degree with the genera *Phanæus* and *Scarabæus* of the *Horæ Entomologicæ*.

† These five names are, *Mesopus, Pleuropus, Merisma, Apus,* and *Resupinatus*.

‡ Published in 1819.

natural

natural group complete in all its parts, there the determinate number is *five*.

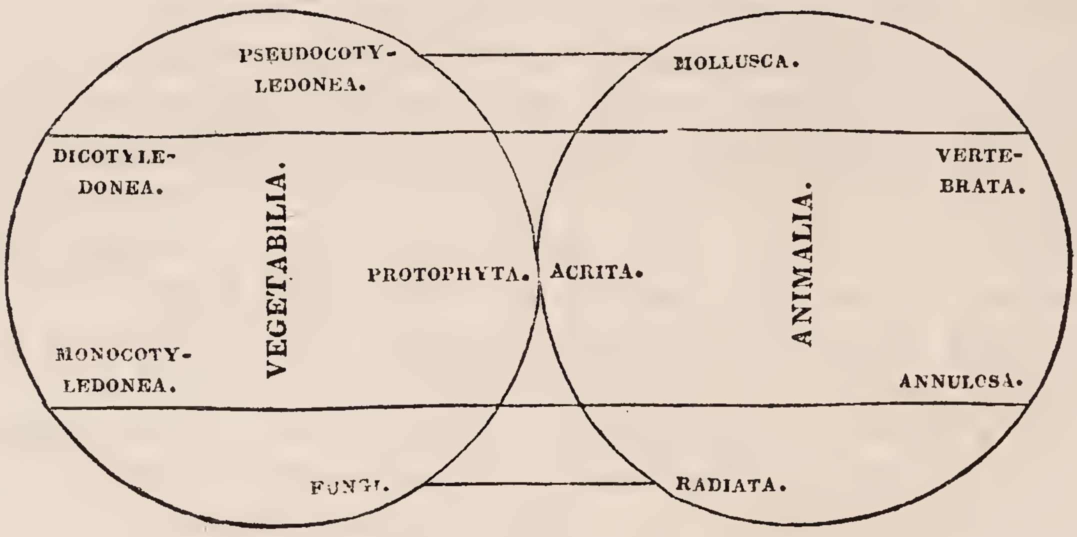
Now, on considering that his work was given to the world two years after the first part of the *Horæ Entomologicæ*, it is clear that, had M. Fries fixed at once on the number five, there might have been room for supposing, that he had not altogether trusted to his own observation, but had borrowed the idea of a quinary distribution. As matters however at present stand, this supposition cannot for a moment be harboured; and I cannot help rejoicing that the strength of this beautiful theory should be so completely brought home to the conviction of every mind, as it must be, by observing the manner in which different persons have respectively stumbled upon it in totally distinct departments of the creation. We may all possibly be wrong in part, or even in much of our respective details; but however this may be, it is difficult not to believe that we are grasping at some great truth, which a short lapse of time will perhaps develop in all its beauty, and at length place in the possession of every observer of nature.

It may be well to note, that M. Fries draws in the clearest manner a distinction between his *Hysterophyta* or *Fungi*, and the *Protophyta*, which is a natural group consisting of the Linnæan *Algæ* and *Lichenes*. He proves that they form two distinct series of vegetables having analogous exterior forms at their corresponding points. Hence, according to what has preceded, the *Protophyta* and *Fungi* form in the vegetable kingdom two primary groups of equal degree. In *Protophyta* fructification is secondary, and the thallus essential; whereas in *Fungi* it is quite the reverse. According to our author, the first-born of Flora may all be accounted as essentially *roots*, and representing the mode of nutrition; while every fungus is as truly and representatively connected with fructification and reproduction. Throwing aside other considerations, we may perceive the analogous groups of the animal kingdom to be likewise constructed on a similar plan. Each of the *Acrita*, for example, imbibing nourishment at every pore of their surface, internal or external, is essentially a stomach, while the situation of the singular ovaries of the *Radiata* cannot fail to remind us of the importance and position of the sporidia in *Fungi*. The umbellate *Medusa*, the *Echinus*, the *Asterias*, and the *Priapulus*, have all their representatives in mycology, of which the genera *Lycoperdon* and *Phallus* are noted instances; so that the analogy of the Radiated animals to *Fungi* is complete; and we thus have in organized matter the following two series of groups connected by affinity and analogous at their corresponding points;

 Animalia.

Animalia.	Vegetabilia.
Acrita	Protophyta
Radiata . . .	Hysterophyta
Annulosa . . .	Monocotyledonea
Vertebrata . .	Dicotyledonea
Mollusca. . . .	Pseudo-cotyledonea? *Agardh**.

Consequently some general idea of the primary distribution of all organized beings may be obtained from the following figure.



To conclude: If an arrangement be natural, it will stand any test; and to support the truth of this proposition, I shall now

* This last department of the vegetable kingdom, *Pseudo-cotyledonea*, has been defined by M. Agardh in the sixth part of his *Aphorismi Botanici*, which is dated December 1821. According to him it embraces the *Musci*, *Hepaticæ* and *Filices* of Linnæus; and in page 76 of the same work we find a comparison made between these plants and *Amphibia*, which is nevertheless much stronger when applied to them and the *Mollusca*. "Pseudo-cotyledoneæ Amphibiis non dissimiles, humum perreptant vel rimas quærunt, humiditateque gaudent ut illa, organis jam in superiore sectione deperditis iterum instructæ." In these last words he alludes to his own opinion, that Mosses display organs nearly related to the cotyledons of dicotyledonous plants, while the monocotyledonous plants conceal their cotyledon; and if botanists should adopt this opinion, we might assimilate it to the curious fact, that in the animal kingdom the imperfectly organized *Mollusca* display a heart, which is more analogous to that of the *Vertebrata* than the dorsal vessel of insects. With respect, indeed, to the analogies existing between the animal and vegetable kingdoms, they are too striking to have altogether escaped the notice of such an observer as Agardh, who truly observes, "Memorabilis est analogia evolutionis seriei vegetabilis cum animali." When we find him, however, comparing the least perfect vegetables to some of the most perfect animals, the *Algæ* to Fishes, and the *Lichenes* to Insects, we must suspect that he is not sufficiently acquainted with the *evolution* of the animal series, and conclude that he has at least not sufficiently attended to the parallelism of analogy. Nevertheless, his comparison of Monocotyledonous, or, as he terms them, of Cryptocotyledonous Plants to Birds, appears to be a true relation of analogy, although an

now arrange Annulose Animals in the same way that M. Fries has distributed his Fungi, when it will readily be seen as virtually nothing else than the arrangement I offered to the public in the *Horæ Entomologicæ*. Thus, it is only necessary that instead of subjecting Nature to arbitrary rules of our own invention, we should humbly receive her laws as she clearly proclaims them; when she will indeed appear, as M. Fries has found her to be, "ubique varia, semper tamen eadem."

Classification of Annulosa *on the same Principles as those adopted by M. Fries in his natural Distribution of Fungi.*

Annulose Animals, which are not hermaphrodite: or the Annulosa of Scaliger, may all be divided into two groups founded on their larva or fœtus state, viz.

1. *Apterous Insects*, having either no metamorphosis in the usual sense of the word, or only that kind of it the tendency of which is confined to an increase in the number of feet.

   These are the Aptera of Linnæus, and comprehend three classes, viz. *Crustacea*, *Arachnida*, and *Ametabola*, which would be termed *Radii* by M. Fries.

2. *True Insects*, being all subject to that kind of metamorphosis which has a tendency to give wings to the perfect or imago state, but never more than six feet.

   These are the Ptilota of Aristotle, and should, according to M. Fries, be termed the *Centrum* of Annulose Animals.

an indirect one; and if he had paid that attention to Entomology which the science really merits, so acute a botanist could not have failed to perceive, that the arguments he gives in support of this last analogy, only receive their full force when they are employed in the comparison of Monocotyledonous Plants with Insects. Thus, in the same page, he states aëriferous cells to be peculiar to Birds in the animal kingdom, evidently not aware that many more animals than are in the whole department of *Vertebrata* would have no means of getting their fluids aërated did not the air enter their bodies and penetrate through every part of them. But on this head Desfontaines long since set the scientific world at rest, when he established the relation of Dicotyledonous Plants to *Vertebrata*, and of Monocotyledonous Plants to *Annulosa*, not on external appearance merely, but on such primary principles of their respective structures, that we may almost term the former tribe of plants Vertebrated, and the latter Annulose. It would scarcely be fair however towards M. Agardh, did we conceal the fact of his being perfectly aware of the analogies which reign both between the Dicotyledonous Plants and the typical group of *Vertebrata*, and between the *Fungi* and *Radiata*. With respect to this last analogy, indeed, the following words are perhaps more explicit than those previously published, p. 211 of the *Horæ Entomologicæ*—"Fungi superiores animalia Radiata ob figuram radiantem, ob superficiem nudam, ob texturam laxam, ob colorem subsimilem non male revocant."

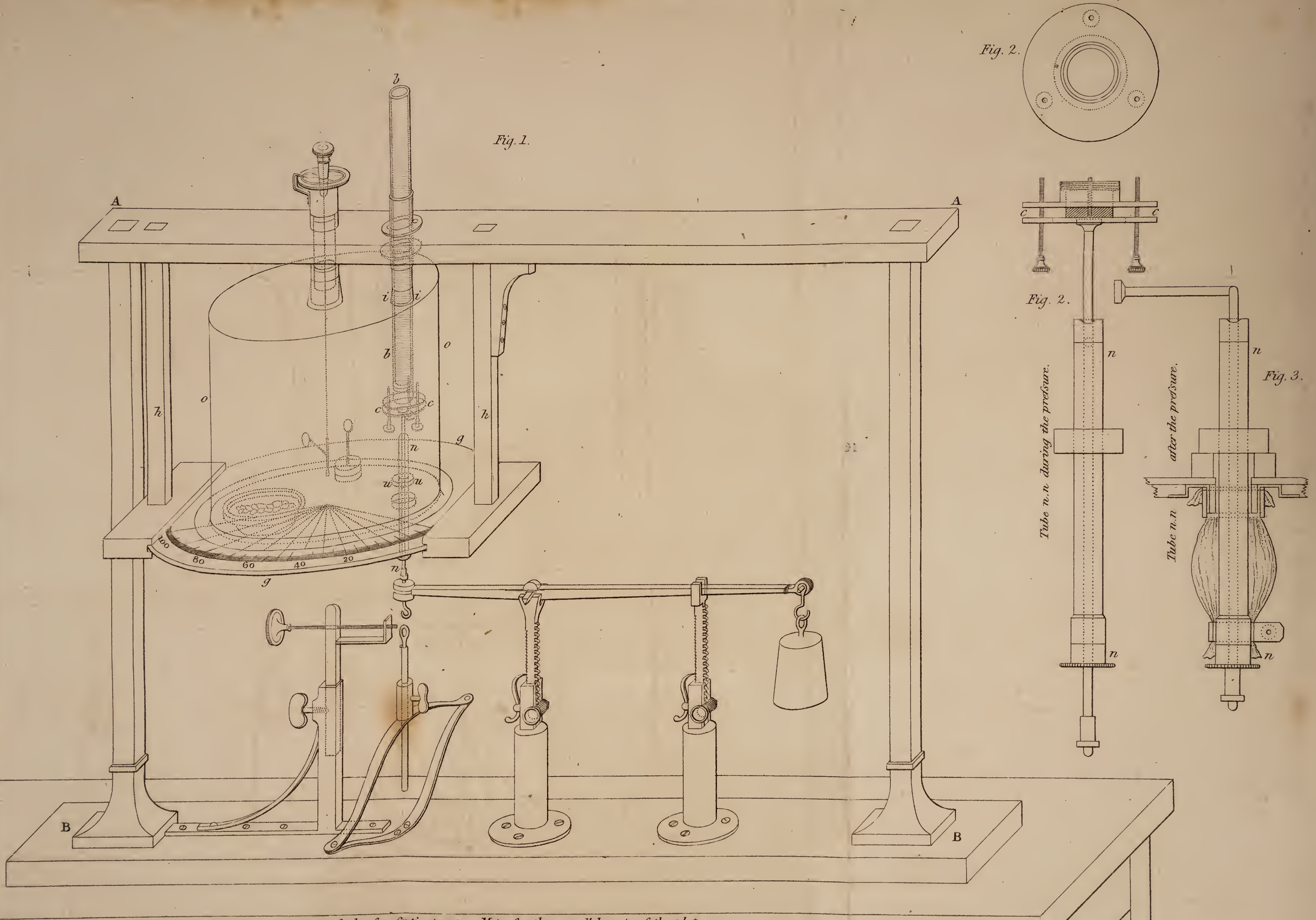
Animals. "*Sed centrum abit semper in duas series,*" and consequently we find that the

PTILOTA

either become by metamorphosis organized for mastication in their perfect state, and are the	or become by metamorphosis organized for suction in their perfect state, and are the
MANDIBULATA of *Clairville,*	HAUSTELLATA of *Clairville,*
which comprise the following orders, viz.	which comprise the following orders, viz.
1. Metamorphosis obtect. Larvæ eruciform. TRICHOPTERA?	1. Metamorphosis obtect. Larvæ eruciform. LEPIDOPTERA.
2. Metamorphosis incomplete, or coarctate. Larvæ apod or vermiform. HYMENOPTERA.	2. Metamorphosis incomplete, or coarctate. Larvæ apod or vermiform. DIPTERA.
3. Metamorphosis incomplete. Larvæ of various types. COLEOPTERA.	3. Metamorphosis incomplete. Larvæ.......................... APTERA. The only larva of this order known is apod or vermiform, but of the coleopterous structure.
4. Metamorphosis semicomplete. Larvæ resembling the perfect Insects. ORTHOPTERA.	4. Metamorphosis semicomplete. Larvæ resembling the perfect Insects. HEMIPTERA.
5. Metamorphosis various. Larvæ hexapod. NEUROPTERA.	5. Metamorphosis various. Larvæ hexapod. HOMOPTERA.

N.B. A mark of doubt is annexed to the word *Trichoptera*, because entomologists have not yet determined whether the Linnæan genus *Phryganea* forms part of an annectent order, or whether it forms a distinct osculant order.

LIII. *Ex-*



Scale of 14 Centimetres to a Metre for the parrallel parts of the plate.

0 1 2 3 4 5 6 7 8 9 10 Decimetres.

S. Porter sc.

LIII. *Experiments on the Development of Electricity by Pressure;—Laws of this Development.* By M. BECQUEREL, *Ancien Chef de Bataillon du Genie.*

[Continued from p. 211.]

Comparison of the electric Phænomena produced by Pressure with those presented by the Exfoliation of certain Mineral Substances.

IT is well known that two laminæ of mica suddenly separated in the dark give out light. No attempt had been made to ascertain any thing beyond this fact; but if the precaution is taken to make the experiment with each lamina fixed to an insulating handle by means of a little mastic, it will be seen that each, in separating, carries with it an excess of contrary electricity, and that this excess is greater in proportion to the velocity of the separation. These results are always obtained, however thin the lamina of mica may be; it is therefore probable that they would still be obtained in separating two molecules from each other. The foliated talc of St. Gothard, and the limpid sulphate of lime of Montmartre, give similar electric results by exfoliation; and it is probable that if we had the means of separating quickly, that is to say, of splitting, all crystallized substances in the manner of mica, each of the parts would retain an excess of contrary electricity; and I doubt not but that the intensity of the electricity developed would bear some relation to the force of the molecular attraction*.

A severed card presents analogous results. It appears, then, that when certain crystallized substances are exfoliated, electrical results are obtained similar to those afforded by two substances withdrawn from compression: the influence of the velocity of separation is equally felt in these two modes of action. Is not the development of electricity to be attributed, in both cases, to the destruction of the *molecular* attraction?

If this be the case, the effect of pressure being to augment that attraction, it would follow that the more strongly these bodies were compressed the greater would be the development of electricity. This is precisely what happens, as we shall have occasion to observe in speaking of the laws which regulate the development of electricity by pressure.

Two laminæ of mica detached from the same piece being put together and pressed anew, remain after pressure in two

* Since this memoir has been drawn up, these phænomena have received much further development; and the new facts which have been observed make known the distribution of electricity in regularly crystallized mineral substances.

different

different electrical states; but this property lasts for a few moments only after exfoliation. If we wish to restore it to them, it is necessary to raise the temperature of one of the laminæ. From this it appears, that when two thin laminæ of mica are abruptly separated, they are not both at the same temperature; since after some instants, that is to say, when an equilibrium of temperature has established itself between the two laminæ, it is necessary slightly to heat the one in order that the pressure may develop electricity. This agrees with our remarks upon what takes place when two bodies identical in all respects are pressed upon each other. It is much to be desired that it could be immediately ascertained by experiment, whether the temperature of each lamina is really the same immediately after their separation.

Sulphate of lime requires certain precautions before it can be rendered electric by exfoliation; it is first necessary to deprive it of its hygrometric water, and afterwards to raise its temperature, in order to make the phænomena apparent. The mode of development of electricity by exfoliation, appears, with very few exceptions, to be adapted only to regularly crystallized substances: it is not the effect of mere rending; for when a tube of glass, or a strip of lac, is broken, each part is totally devoid of electricity.

Laws of the Development of Electricity by Pressure.

We are ignorant, as yet, whether the cause of electric phænomena is a species of matter emitted, or whether it is merely the result of a vibratory movement impressed on the molecules of bodies: the uncertainty which prevails on this subject proves that the phænomena relative to the development of electricity are still covered by a thick veil. Many important physical properties of electricity are already known; among others its attractions and repulsions, and the laws according to which they take place; its distribution over conducting bodies, whether insulated, or subject to the influence of other electrified bodies: but no investigation has yet been made into the laws of the development of the electric principle; an inquiry of that nature demands a mode of electrifying at once simple and easily measurable; such a means is found in pressure.

We have shown, by a great number of experiments, that two bodies conveniently disposed, and pressed against each other, were found, when withdrawn from compression, to be in different electric states; that if exceptions to this rule were found to exist, they proceeded merely from the want of that degree of velocity in separating the bodies, which is necessary to prevent the two fluids from recombining; and that in

in the case of two perfect conductors, the velocity of separation ought to be infinite.

The development of electricity by pressure is modified, as we have seen, by the nature of the bodies; by the state of their surfaces; by their hygrometrical condition, the degree of the pressure, the velocity of the separation, and the temperature. It is necessary then to study the influence of each of these causes, in this development, if we desire to discover the electric phænomena which relate to them.

The researches concerning the development of electricity have hitherto been confined to the discovery of the means of setting in motion the electric principle, and the inquiry into the circumstances under which this phænomenon was modified; but it has never been attempted to measure its effects when one of the influential causes varied. It is certainly something to discover a phænomenon; but the question is only half resolved, if the law according to which it operates is not also discovered; this law comprehends in its extent all the particular cases, which then become immediate consequences of it.

We know, for instance, that friction, heat, evaporation, &c. are so many agencies by which electricity is disengaged; but what is the intensity of this disengagement when the friction is more or less rapid, when the temperature is more or less elevated? Of this we are still in ignorance. Friction, which is a compound phænomenon, is less adapted to inquiries of this nature than pressure, which is a more simple mode of action. We can, in fact, increase or diminish its intensity by a determinate value; and on comparing the quantities of electricity which result from it, we must deduct from them the relation between the pressures and the corresponding electric intensities. This relation constitutes one of the laws of the development of electricity by pressure. I shall take occasion to observe, in the sequel, that pressure being one of the elements of friction, it is natural to examine, in the first place, the nature of its action upon the phænomena, in order to be afterwards qualified to draw some inferences concerning the development of electricity by friction.

Description of the Apparatus for measuring the electric Effects produced by Pressure.

Inquiries like that in which we are engaged, require an apparatus by means of which the causes which influence the development of electricity may be varied, and their effects measured, at pleasure: for the present, however, we shall attend to the variation of one only of the influential causes, and shall sup-

pose all the others constant. We shall therefore act upon bodies of which the polish, temperature, and hygrometrical state are sensibly the same: we shall only vary the pressure. It would otherwise be impossible, in the actual state of knowledge on the subject of the development of electricity, to distinguish, in the general effect, the part which each of the influential causes would have had in the production of electricity, &c.

The following apparatus fulfils the necessary conditions: we take an electric balance which is placed on a horizontal shelf, *g g*, fig. 1, covered by a glass; this shelf is suspended by means of two vertical pieces *h h* to the cross-piece A A of a strong wooden frame A A B B. The glass cover *o o* of the balance is pierced at its upper end by an aperture *i i*, through which is passed a copper tube which descends within it, and which is made fast to the horizontal cross-piece by strong screws. At the extremity of this tube is fixed an apparatus *c c*, composed of two small circular plates of copper, which may be brought together by means of three screws, and the lower of which has an aperture of two centimetres in diameter; between these two plates is placed the body which is to be subjected to pressure. This little apparatus communicates with the common reservoir by means of a metallic chain for carrying off the electricity. When it is desirable to raise the temperature of the body, a liquid heated to the requisite degree is poured into the copper tube *b b*. Two apertures are also made in the shelf *g g*; one, of about a decimetre in diameter, for the internal use of the balance, may be closed at will by a glass capsule, upon the edge of which is a bayonet joint (*douille à vis*); this capsule is also intended to receive the substances for absorbing the humidity of the inside of the balance; the other aperture *u u* serves as a passage for a small glass tube, covered with lac varnish, and carrying at its upper extremity the body which is to press that placed between the two circular plates: when it is intended to apply pressure, this tube is placed, by means of a little foot, upon one of the extremities of the beam of the balance, the other being suitably disposed for the reception of the weights necessary for the production of the pressure. The aperture made in the shelf is sufficiently large not to impede its movements; and in order to prevent the communication of the external air with that inclosed in the case of the electric balance, a cylinder of gold-beater's skin is fixed, by means of a ring, around the aperture, in the middle of which the little tube passes, and the dimensions of which are such that the light skin of which it is formed does not in any degree impede the movements of the little tube. The

beam

beam of the balance rises and falls at will by means of a screw. The apparatus is then placed in such a manner, that the beam may be in a horizontal direction when the two bodies subjected to pressure just touch each other. Things being thus arranged, the pressure is applied; after which, in order to withdraw the bodies from pressure, two springs are used, disposed in such a manner as to receive a determinate tension. The springs, returning to their original state, draw with them the beam; the bodies are therefore withdrawn from compression with a velocity equal to the tension of the springs. The body placed at the extremity of the tube being withdrawn from compression, it is necessary to present it to the disk of gilt paper in the direction of its greatest surface; which is accomplished by forming this tube of two pieces and joining them by means of bolting joints (*charnière à boulon*): then inserting this tube, which is solid, into another a little larger, the two parts are in a right line. If it is desired to present the body to the disk of gilt paper, the smaller tube is drawn a little way out of the one in which it is inclosed, and the upper part immediately slips down.

Most frequently the excess of electricity acquired by each of the substances, on the cessation of the pressure, is very small; consequently, if a silver wire like that used by Coulomb in his experiments be employed as the wire of torsion, the repulsion would be scarcely sensible, and sometimes null; we must therefore substitute another whose force of torsion is much weaker. Very fine platinum wire drawn according to Dr. Wollaston's plan, perfectly answers this purpose: but care must be taken to choose the proper degree of fineness; for I have ascertained that when these wires have attained a certain degree of tenuity, the torsion of a small angle deranges the aggregation of the molecules so much, that they do not recover their primitive position of equilibrium. The result is, that the oscillations of a horizontal pendulum suspended to the extremities of this wire are no longer isochronous; it must therefore be rejected.

One of the bodies is placed between the two copper plates; polished on one of its surfaces, if it is mineral and susceptible of being polished; the other, for which cork or elder-pith is usually taken in comparative experiments, is in the form of a small disk of very little thickness, and of the same diameter as the disk of gilt paper of the balance. It follows, that when these two disks, after having both been brought to zero, are in contact, they possess an equal share of electricity: repulsion immediately takes place, and the effect is measured by means

 of

of the circumference of a divided circle traced on the horizontal shelf.

The circumstance of the case being more or less cylindrical, can in no respect alter the value of the degrees. The position of the arm of the lever which carries the disk of gilt paper, is exactly determined by means of a vertical rule placed on a circular foot, which is passed along the circumference of the circle, and whose centre corresponds to the prolongation of the wire of torsion.

Let us now see the effect of the equal division of electricity between the two disks. It is well known that the total force of the repulsion varies, at each distance, in the same ratio as the quantities of electricity which contribute to this repulsion: it follows therefore that the expression of its force, which is called Electric Reaction, must be in proportion to the product of these two quantities. Now let x represent the excess of electricity acquired by the disk of cork or of elder-pith at its escape from compression; immediately after contact with the disk of gilt paper both will possess an excess of electricity $\frac{1}{2}x$; the electric reaction will then be expressed by $\frac{1}{4}x^2$: this same reaction is however given directly by experiment, since the arc of the circle which measures the distance of the two disks is in proportion to it, in so far as that arc may be taken for its chord; which may always be done by twisting sufficiently the suspending wire.

Let e be this arc, we shall have $e = \frac{1}{4}x^2$; whence $x = 2\sqrt{e}$. This is the value of the quantity of electricity acquired by the disk of elder-pith after a pressure P. For another pressure P′ we shall have $x' = 2\sqrt{e'}$; but if in the two experiments the two disks are brought back to the same distance by means of torsion, the values of x and x' will become comparable, and we shall deduce from them the ratio of the electric intensities attributable to different pressures.

This manner of determining the quantity of electricity produced by any pressure whatever, may be employed whenever the repulsion of the disk of gilt paper, after the division of the electricity, is measured by an arc of a certain number of degrees; but the development of electricity is frequently so feeble that the electric reaction becomes unappreciable, notwithstanding the great sensibility of torsion of the platinum wire. We must therefore have recourse to another expedient: for this purpose, it is sufficient to give to the disk of gilt paper a quantity of electricity whose intensity may be determined at every experiment; all the results will then become comparable; a second disk of gilt paper of the same diameter as the first, and insulated

sulated in the same manner, is placed in the case of the balance; it is electrified by means of a brass wire which crosses the case: the two disks of gilt paper are then brought into contact by turning the screw which carries the micrometer; an equal division of electricity immediately takes place, and is followed by repulsion. The micrometer is again turned to bring back the two disks to a distance measured by a small arc, one of ten degrees, for instance, which may be taken for its chord.

Let a be the number of degrees of torsion of the wire, $\sqrt{10^\circ + a}$ will represent the quantity of electricity possessed by each disk of gilt paper. After making these arrangements, let us withdraw the two bodies from compression, and place the disk of cork, which is supposed to have acquired an excess of electricity of the same nature as that communicated to the disk of gilt paper, at zero of the horizontal circle, at the same time bringing back to it the disk of foil which possesses a quantity of electricity $\sqrt{10^\circ + a}$; repulsion will take place. Let us bring back the two disks to an angular distance of 10° by torsion, and let d be the number of degrees of torsion to which we must subject the wire for that purpose; we shall then evidently have, representing by y the excess of electricity possessed by the disk of cork or elder-pith,

$$y . \sqrt{10 + a} = 10 + d; \text{ whence,}$$

$$y = \frac{10 + d}{\sqrt{10 + a}};$$

this is the expression of the quantity of electricity produced by a pressure P. For another pressure P′ we shall have

$$y' = \frac{10 + d'}{\sqrt{10 + a'}};$$

y', a', d', being the quantities analogous to y, a, d. Therefore, the electric intensities arising from the pressures P and P′ are in relation to each other as

$$\frac{10 + d}{\sqrt{10 + a}} : \frac{10 + d'}{\sqrt{10 + a'}}.$$

Use of the Apparatus.

The surfaces of the bodies subjected to pressure ought to be as nearly as possible in the same state of polish; without this precaution, the electric results would not admit of comparison, since the greater or less degree of polish has a remarkable influence on the quantity of electricity developed. If they be mineral substances, they should be cut into thin plates, and brought to the highest degree of polish that art can attain; or, which is preferable, they may be cleaved or split naturally, when

when they possess that property: the surface which is to be subjected to pressure is then rubbed with alcohol to remove any greasy matter which might be found upon it, and the body is suffered to remain for some time in the dry air of the balance: by this means the small stratum of moisture which usually adheres to the surface of all bodies is removed. As to the substances which are not mineral, it is sufficient to deprive them of their hygrometrical water. This latter precaution is indispensable; for certain substances, as, for instance, crystallized sulphate of barytes, mica, sulphate of lime, &c., only become electric by pressure in proportion as they have been previously dried.

At the moment in which the two bodies are placed upon each other, the greatest care must be taken that they undergo no friction, since that would produce a complication of effects for which it would be impossible to account. This is effectually prevented by placing the bodies in such a manner, that the beam of the balance experiences no oscillation in any direction. For this purpose a vertical stem is fixed to the foot of the balance, which rises and falls by means of a rack and nut, the rack terminated at its upper extremity by a fork in which is placed one of the arms of the beam. This rack and nut is so placed that the beam can rise and fallwithout lateral oscillations. Further, to be quite certain that the slightest friction has not influenced the quantity of electricity arising from the pressure, the pressure is allowed to continue for some time*.

It is very difficult to determine the law of electric intensities caused by equal pressure and unequal degrees of velocity of separation; the researches which that law demands cannot be made with the apparatus we are using, but we easily find the law of the electric intensities which result from different pressures and velocities of separation giving the *maximum* of effect. Let us suppose then, that by the preliminary experiments we have determined the *maxima* velocities, and let us see what happens when the pressures increase. Let us take different substances and press them all with the same disk of cork.

* In the experiments, the object of which is to determine the relation between the electric densities and the corresponding pressures, we must not subject to pressure substances in which slight alterations of the surfaces pressed would occasion great differences in the quantities of electricity developed. For instance, cork and elder-pith are improper, because the smallest change of temperature in either of these bodies is frequently sufficient to modify considerably the development of electricity; such bodies as sulphate of barytes and cork must be taken.

Sub-

Substances pressed.	Iceland Spar naturally split (cleaved); Cork.				
Pressures.	Velocities giving the *maximum.*	Values of a.	Values of d.	Electric intensities by the formula $\frac{10+d}{\sqrt{10+a}}$	Means.
1	......	...	...	....	(1·5)
2	......	20	10	3·6	3·4
2	......	26	9	3·2	
3	......	28	16	4·2	4·6
3	......	6	10	5·0	
4	......	6	14	6·2	6·0
4	......	10	16	0·5	

We see then that for the pressures

	1	2	3	4,
the electric intensities are	x	3·4	4·6	6.

The electric intensities are then sensibly proportional to the pressures; for if we suppose $x = 1{\cdot}5$, we shall have results which would differ very little from those given by experiment.

Pressures	1	2	3	4,
Calculated elect. int.	1·5	3·0	4·5	6.

Polished Iceland spar, subjected to the same experiments as cleaved Iceland spar, has constantly given, for the same pressure, a quantity of electricity weaker than the latter, in the relation of two to six; that is to say, that in polished Iceland spar the electric faculty, by pressure, is about one third of what it is in the same substance in its natural state. This difference is remarkable, for the polish of mineral substances usually increases the electric faculty; while in Iceland spar it diminishes it. The hygrometric state had no influence in this difference, since the crystal was deprived of water before the experiment.

Let us submit other substances to pressure:

Substances pressed.	Crystallized Sulphate of Barytes polished; Cork.					
Pressures.	Velocities	Values of a.	Values of d.	Electric intensities by the formula $\frac{10+d}{\sqrt{10+a}}$		Calculated electric intensities.
1	......	...	...	....		1·05
2	......	10	1	2·0		
2	......	10	0	2·2	2·1	2·1
2	......	15	0	2·0		
3	......	15	6·0	3·2		
3	......	15	5·5	3·1	3·1	3·1
3	......	6	2	3·0		
4	......	15	12	4·4		
4	......	20	13	4·2	4·2	4·2
4	......	0	3	4·2		
6	......	5	14	6·1	6·1	6·3
6	......	10	18	6.2		

We again see, by the results exhibited in this table, tha
the electric intensities increase in proportion to the pressures.

Substances pressed :—Hyaline Quartz polished; Cork.						
Pressures.	Velocities	Values of a.	Values of d.	Electric intensities by the formula $\frac{10+d}{\sqrt{10+a}}$		
4	......	1	3	4·1		
4	......	30	29	3·3	3·9	
4	......	4	6	4·2		
4	......	26	15	4·1		

Substances pressed :—Sulphate of Lime; Cork.						
Pressures.	Velocities	Values of a.	Values of d.	Electric intensities by the formula $\frac{10+d}{\sqrt{10+a}}$		
4	......	26	3	2·1		
4	......	20	0	1·8	1·9	
4	......	6	2	2·0		
4	......	6	2	2·0		

Le

Let i, i', i'', i''', be the electric intensities of Iceland spar, of sulphate of barytes, of hyaline quartz, and of sulphate of lime; we shall have for the same pressure, $i : i' : i'' : i''' : : 6 : 4, 2 : 3, 9 : 1, 9$. We see that the electric power in sulphate of lime is about three times less than in Iceland spar; that is to say, that these two substances, under the same pressure of the elder-pith*, retain an excess of positive electricity, which is three times as strong in the Iceland spar as in the sulphate of lime.

These experiments show that the electric intensity for pressures of from one to ten kilogrammes is in proportion to the pressure; that is to say, that for a double pressure the intensity is double, and so on; supposing always that the velocity of separation is such in each experiment as to give the maximum of electric intensity.

Does this proportionality extend to much stronger pressures, using always the requisite velocities? This question is very difficult to answer, for the apparatus with which we operate will admit of only very inconsiderable pressures: nevertheless, if the development of electricity be owing, as appears probable, to the compression of two bodies, that is to say, to the approximation of molecules, we may infer that this development must cease when the molecules have attained a certain degree of compression. In fact, the state of the molecules in bodies may be compared to the force of a spring. It follows, then, that the more bodies have been compressed, the less susceptible will they be of fresh compression, and therefore that a point must exist at which the molecules can be brought no closer. If this be really the case, electric intensity ought to increase rapidly at first, and the rate of its increase should diminish slowly. The preceding experiments show, that under slight pressures the electric intensities form an increasing geometrical progression.

Supposing the temperature to be constant, the electric intensity for any pressure p would have an expression of the form

$$ap \pm A\sqrt{\frac{m}{p}},$$

in which a, A and m would be three constant quantities for the same body, but variable from one body to another; m should be such that for inconsiderable values of p the term

$$A\sqrt{\frac{m}{p}},$$

might be neglected as far as relates to $a\,p$. If in Iceland spar naturally split $a = 1{\cdot}5$, we should have

$$i = p.\,1{\cdot}5 \pm A\sqrt[m]{p}$$

This formula is quite empirical.

* There is a discrepancy between this statement and that in the preceding tables, in which *cork*, and not *elder-pith*, is uniformly mentioned as the substance employed.—Edit.

In considering the manner in which the development of electricity increases in bodies by the augmentation of pressure, ought we not to refer to this cause certain luminous phænomena, the origin of which is as yet imperfectly known? It is said, for instance, that in the polar seas it often happens that light is elicited by the shock of blocks of ice. These enormous masses come in contact with a considerable quantity of motion; they must therefore experience great compression, which induces upon each of them two different electric states. At the moment when compression ceases, the two fluids immediately recombine on account of the conductibility of the ice. Is not the light disengaged to be attributed to the sudden recomposition of these fluids?

Iron struck with repeated blows also becomes luminous: are not the same electric phænomena of pressure produced in this instance, as when two masses of ice are dashed together?

Summary.

In reviewing what we have just laid before our readers, it will be seen, 1° that all bodies acquire two different electric states by pressure;—that in two bodies, perfect conductors, this state of equilibrium ceases at the moment in which the pressure is withdrawn; whereas when one of the bodies is not a good conductor, the effect of the pressure continues for a longer or shorter duration of time;—that pressure alone preserves the equilibrium of the two fluids placed upon each of the surfaces; since, when there is any diminution of the pressure, and at the end of a certain time the bodies are withdrawn from compression, neither of them retains more than the quantity of electricity due to the remaining pressure;—that caloric modifies the development of electricity in a manner perfectly peculiar to itself;—that the electric intensity increases at first in the direct ratio of the pressure, and that it is probable that this relation diminishes in high pressures, in the same degree as bodies lose their compressibility:—lastly, we have seen, that it is probable that the light disengaged in great shocks is owing to the sudden recomposition of the two fluids, which had been developed upon each surface at the moment of compression.

LIV. *A few Observations on the Natural Distribution of animated Nature. By* A Fellow of the Linnean Society.

[Continued from p. 202.]

HEREWITH I send you a continuation of my tabular distribution of Nature, containing the Animal Kingdom: but it is given with diffidence, and rather with a view to excite examination, than with a due conviction of any real service it is capable of affording even to a naturalist; and by such a reader only will it be easily comprehended. Yours, &c.

October 1823. F.L.S.

Erratum.—In the printed table inserted in your last Number, the words Dicotyledonous and Monocotyledonous have been transposed, owing to a mistake in transcribing. It is necessary that the reader should correct this, as the error it gives rise to is very important.

Natural Distribution of Animated Nature.

1. VERTEBRATE. *Continued from p. 202.*

Gilled — Lunged

Gilled: Oviparous — Viviparous
Fishes — *Reptiles*
Fishes: Apode — Subpedate.
Reptiles: Apode — Pedate
Pedate: 2-footed — 4-footed.

Lunged: Oviparous — Viviparous

Oviparous, *Birds*: Land — Water
Land: Rapacious — Phytophagous
Rapacious: Day + Night
Night: Eared — Earless.
Phytophagous: Hard-billed — Soft-billed.

Viviparous, *Mammalia*: Subapode — Pedate
Subapode: Whales — Seals.
Pedate: 4-handed — 4-footed
4-handed: Mankind — Monkeys
Monkeys: Tailed — Tailless
Long-tailed — Short-tailed
Prehensile-tailed — Not prehensile.
4-footed: Carnivorous — Herbivorous
Herbivorous: Ruminant — Not ruminant.

2. INVERTEBRATE. *Continued from p. 202.*

Vermes, Linn. — *Insects*, Linn.

Vermes, Linn.: Molluscous — Testaceous
Molluscous: Internal — External
Internal (*i. e.* Intestinal): Pertused — Imperforate.
External: Zoophyte — Lithophyte
Zoophyte: Fixed — Locomotive.
Testaceous: Land — Water
Water: Univalve — Plurivalve
Plurivalve: Bivalve — Multivalve.

Insects, Linn.: Wingless — Winged
Wingless: Crabs — Spiders.
Winged: Mandibulate — Haustellate
Mandibulate: Hard-winged — Soft-winged
Hard-winged: Coleopterous — Orthopterous
Coleopterous: Land — Water.
Haustellate: Tetrapterous — Dipterous.
Tetrapterous: Hard-winged — Soft-winged
Hard-winged: Land — Water.
Soft-winged: Stinging — Stingless
Stingless: Powdery — Not powdery
Powdery: Day + Night, Lepidoptera.
Not powdery: Bomboptera.

LV. *True apparent Right Ascension of* Dr. Maskelyne's *36 Stars for every Day in the Year* 1823, *at the Time of passing the Meridian of Greenwich.*

[Continued from page 191.]

1823.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 53	H. M. 4 25	H. M. 5 3	H. M. 5 6	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 30	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 40	H. M. 11 41	H. M. 13 15	H. M. 14 7
Nov.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	12·46	18·08	7.08	51·45	44·08	6·36	11·96	39·97	24·38	22·12	5·86	32·91	56·26	59·36	4·08	31·21	55·09	37·30
2	45	08	09	47	11	38	99	40·00	41	16	90	94	29	39	10	23	11	31
3	45	09	11	49	14	40	12·02	02	44	19	93	98	32	42	13	26	13	32
4	44	09	12	51	18	43	05	05	47	23	96	33·01	35	45	15	28	14	33
5	44	10	13	53	21	45	07	08	49	26	99	05	38	48	17	30	16	34
6	43	11	14	55	24	47	10	10	52	30	6·02	08	41	51	20	33	18	35
7	42	11	15	57	27	49	13	13	55	33	03	12	44	54	23	36	20	36
8	42	11	16	59	30	51	15	15	58	37	06	15	47	57	25	38	22	37
9	41	12	17	61	33	53	18	17	60	40	09	18	51	60	28	41	23	39
10	40	12	18	63	36	55	20	20	63	44	12	22	54	63	30	44	25	40
11	40	12	19	65	39	57	23	22	66	47	15	25	57	66	33	46	27	41
12	39	13	19	67	42	59	25	25	68	51	18	29	60	70	36	49	29	43
13	38	13	20	69	45	61	28	27	71	54	21	32	63	73	38	52	31	44
14	38	13	21	70	48	63	30	29	74	58	24	35	67	76	41	54	33	46
15	37	14	21	72	50	64	33	32	76	61	27	39	70	79	44	57	36	47
16	36	14	22	74	53	66	35	34	79	65	32	42	73	82	47	60	38	49
17	35	14	23	76	56	68	37	36	82	69	35	45	76	85	50	63	40	51
18	34	14	23	78	58	70	40	39	84	72	38	49	79	89	53	66	43	52
19	33	15	24	79	61	71	42	41	86	75	41	52	83	92	56	68	45	54
20	33	15	24	81	64	73	44	43	89	79	44	55	86	95	59	71	48	56
21	32	15	25	82	66	75	46	45	91	82	46	59	89	98	62	74	50	58
22	31	15	25	84	68	76	49	47	94	85	49	62	92	60·02	65	77	52	60
23	30	14	26	85	71	78	51	50	96	88	52	65	95	05	68	80	55	62
24	29	14	26	86	73	80	53	52	98	92	55	69	99	08	71	83	58	64
25	28	14	27	88	76	81	55	54	25·01	95	58	72	57·02	12	74	86	60	66
26	27	14	27	89	78	83	57	56	03	98	61	75	05	15	77	89	63	68
27	26	14	27	90	80	85	59	58	05	23·01	64	78	08	18	80	92	66	70
28	25	14	28	92	83	86	61	60	08	04	67	81	11	22	83	95	69	72
29	24	14	28	93	85	88	63	62	10	07	69	84	14	25	86	98	71	74
30	23	14	28	94	87	89	65	64	12	10	72	87	18	28	89	32·01	74	77

1823.	1α Libræ.	2α Libræ.	α Cor. Bor.	α Serpentis.	Antares.	α Herculis.	α Ophiuchi.	α Lyræ.	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1α Capri.	2α Capri.	α Cygni.	α Aqua.	Fomalhaut.	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 56	H. M. 23 59
Nov.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.	S.
1	57·22	8·64	13·37	35·49	37·08	36·88	45·41	58·18	53·55	11·75	40·17	53·50	17·28	26·14	45·34	55·76	1·14	19·92
2	23	65	37	49	08	87	40	16	53	73	15	48	26	11	33	75	12	92
3	24	66	37	49	08	86	39	14	52	72	14	47	25	09	31	74	11	91
4	* 25	* 67	37	50	08	86	38	12	50	70	13	45	23	06	30	73	10	91
5	26	68	36	50	07	85	38	10	49	69	11	44	22	04	29	71	09	90
6	27	69	36	50	07	84	37	08	47	67	10	42	20	01	28	70	08	89
7	28	70	36	50	07	83	36	06	45	66	09	41	19	25·98	27	69	07	88
8	29	71	36	51	07	83	36	04	44	64	07	39	17	96	25	67	06	87
9	30	72	37	51	07	82	35	03	43	63	06	38	16	94	24	66	05	86
10	32	74	37	52	08	82	34	01	41	61	05	36	14	91	23	64	04	86
11	33	75	37	52	08	81	34	57·99	40	60	03	35	13	89	22	63	03	85
12	34	76	38	53	08	81	33	98	39	59	02	34	12	86	20	61	02	84
13	35	77	38	54	08	81	33	96	38	58	01	33	11	84	19	60	00	83
14	37	79	39	54	09	80	33	95	36	56	39·99	31	09	82	18	58	0·99	82
15	38	80	39	55	09	80	32	93	35	55	98	30	08	79	16	57	98	81
16	40	82	40	56	10	80	32	92	34	54	97	29	07	77	15	55	97	80
17	42	84	41	57	10	80	32	90	33	53	96	28	06	75	14	53	96	79
18	43	85	42	58	11	81	32	89	32	52	95	27	05	72	12	52	95	78
19	45	87	43	59	12	81	32	88	31	51	94	26	04	70	11	50	93	77
20	47	89	44	60	13	81	32	87	30	50	93	25	03	68	10	49	92	76
21	48	90	45	61	13	81	32	85	29	49	92	24	02	66	08	47	91	75
22	50	92	46	62	14	81	32	84	28	48	91	23	01	64	07	45	90	74
23	52	94	47	63	15	81	32	83	28	47	90	22	00	61	06	44	89	73
24	54	96	48	64	16	81	32	82	27	46	89	21	16·99	59	05	42	87	72
25	56	98	49	65	17	81	31	81	26	46	88	20	98	57	03	41	86	71
26	58	9·00	51	67	18	81	31	80	25	45	87	19	97	55	02	39	85	70
27	60	02	52	68	19	81	31	79	24	44	86	18	96	53	01	37	84	69
28	62	04	54	70	21	82	31	78	23	43	85	17	95	52	00	36	83	68
29	64	06	55	71	22	82	32	77	22	43	84	16	94	49	44·98	35	81	67
30	66	08	57	73	24	83	32	76	22	42	84	16	94	47	97	33	80	65

N.B. On those days where an Asterisk is prefixed the Star passes twice; the Æ given is that at the first passage.

LVI. Con-

LVI. *List of Occultations for the Year* 1824, *computed for the Meridian and Parallel of* Greenwich. *By M.* Inghirami *of Florence.*

[Continued from p. 165.]

Day.	Star.	Mag.	Cat.	Æ	D	Im.	Em.	Dist. Im.	Dist. Em.
						MAY.			
				° ′	° ′	h m	h m	′	′
1	Tauri	6	P 295	73 59	23 59 N	10 27	11 11	1 S	0
2	Gemin.	8	Z 282	88 28	23 39	9 28	10 2	12 S	8 S
—	2———	6·7	P 323	88 39	23 39	9 43	10 22	11 S	6 S
3	———	7	L 9	104 24	21 36	10 50	11 37	9 S	1 S
4	Cancri	7·8	P 273	117 17	18 47	7 48	cont.		
—	———	7	L 8	117 23	18 49	7 48	8 32	15 S	7 S
—	16———	6	P 5	120 11	18 14	12 19	12 50	9 N	16 N
6	Sextant.	8	P 202	145 53	9 1	8 40	9 46	13 S	3 N
—	———	7·8	P 208	146 20	8 37	10 2	10 44	16 S	15 S
7	36———	6	P 147	158 43	3 32	7 38	8 50	7 S	11 N
9	Virginis	7	P 142	187 3	2 57	13 41	14 41	11 S	1 N
11	———	7·8	L 10	213 26	19 0 S	13 13	13 37	12 N	16 N
—	———	7	L 10	213 27	19 0	13 15	13 37	12 N	15 N
13	Scorpii	7·8	P 14	240 42	24 57	10 53	11 14	15 S	13 S
—	———	7	L 13	240 30	24 56	10 57	cont.		
—	σ———	4	P 50	242 16	25 6	14 6	15 2	10 S	7 S
15	Sagitt.	7	L 13	268 56	25 29	10 48	11 25	13 S	13 S
—	———	7	L 13	268 59	25 29	10 54	11 30	13 S	13 S
—	———	7	L 13	268 59	25 29	10 54	11 30	13 S	13 S
16	———	8·9	P 253	282 19	23 30	10 20	10 55	13 N	12 N
—	———	7	L 13	284 1	23 30	14 16	15 34	1 S	7 S
—	———	8	L 13	284 13	23 30	14 50	15 58	4 S	9 S
17	———	8	L 13	297 4	20 25	15 18	16 14	4 S	9 S
19	Aquarii	6·7	L 13	320 14	13 11	13 17	14 20	1 N	10 S
21	———	8·9	P 269	342 23	13 30	13 35	14 33	1 N	10 S
24	Piscium	7·8	L 11	16 53	12 1 N	15 17	15 26	14 N	13 N
25	Arietis	7	L 10	29 28	16 14	14 48	15 39	8 N	2 N
30	d Gemin.	6·7	P 247	99 53	21 59	10 6	10 49	6 S	4 N
						JUNE.			
				° ′	° ′	h m	h m	′	′
2	Sextant.	7	L 10	143 31	9 50 S	11 13	11 52	5 N	14 N
3	———	7·8	L 10	156 12	4 17	10 37	cont.		
4	Leonis	7·8	L 10	169 37	1 50	10 49	11 49	8 N	7 S
—	———	4·5	P 89	170 1	1 54	11 20	12 15	1 S	13 S
5	Virginis	7·8	L 10	181 42	7 36	9 44	10 20	15 S	8 S
—	———	8	P 35	182 4	7 47	10 18	10 56	16 S	10 S
—	14———	6·7	P 41	182 16	7 48	10 36	10 54	14 S	5 S
8	Solitar.	7·8	L 10	221 21	21 17	9 6	10 17	2 S	8 S
—	———	7·8	L 10	221 40	21 33	10 0	11 6	10 S	6 S
9	Scorpii	6	P 191	235 30	23 55	9 17	10 11	6 N	12 N
—	———	6	L 13	235 31	23 57	9 33	10 35	5 N	11 N
—	———	6·7	L 13	236 30	24 13	11 52	13 3	1 S	3 N
—	———	7	L 12	237 34	24 25	14 15	15 16	4 S	3 N
10	25 Scorpii	6	P 168	248 39	25 9	7 9	8 8	2 N	6 N
—	———	7	L 13	249 48	25 14	9 52	11 1	7 N	11 N
—	———	7	L 13	249 51	25 13	10 5	11 1	8 N	11 N
—	———	7·8	L 13	250 22	25 28	11 16	12 14	3 S	1 S
12	Sagitt.	8	Z 1241	277 41	24 0	10 15	10 44	15 N	14 N
14	Capric.	7·8	P 80	302 23	18 57	9 20	10 16	9 S	2 N
—	———	8	P 97	302 55	18 58	10 21	11 26	0	7 S

Day.	Star.	Mag.	Cat.	Æ	D	Im.	Em.	Dist. Im.	Dist. Em.
				° ′	° ′	h m	h m	′	′
14	π Capric.	5	P 131	303 58	18 51 S	13 27	cont.		
—	ϱ ———	5	P 142	304 22	18 28	14 6	15 23	3 N	8 S
—	———	7·8	P 144	304 24	18 31	14 10	15 20	1 S	11 S
15	———	7	L 8	316 25	14 9	14 14	15 24	12 N	0
16	Aquaii	7·8	L 10	326 37	10 33	11 14	11 33	11 S	15 S
—	———	7·8	L 10	327 58	9 30	15 3	16 6	13 N	1 N
18	κ² Piscium	6	P 84	349 15	0 2 N	13 8	14 5	7 S	0
23	Tauri	6·7	L 8	51 36	21 58	14 3	14 36	13 S	7 S
				JULY.					
				° ′	° ′	h m	h m	′	′
1	Leonis	7·8	L 10	165 9	0 15 S	8 36	cont.		
6	Libræ	6	L 13	232 58	23 43	12 11	13 12	3 S	0
9	25 Sagitt.	7·8	P 108	275 35	24 22	14 39	15 33	12 N	7 N
10	———	8	L 13	286 6	22 51	8 29	9 23	10 N	7 N
—	———	6	P 61	287 10	22 46	10 32	11 45	5 N	1 S
—	———	6	L 13	287 12	22 40	11 10	12 18	10 N	2 N
—	———	7	L 13	287 25	22 27	12 21	cont.		
—	50 ———	6·7	P 103	288 36	22 9	14 39	15 12	14 N	10 N
—	———	8	L 13	288 39	22 1	15 21	cont.		
11	———	8	P 388	298 39	20 3	7 47	8 52	8 N	0
—	Capric.	8	L 13	299 16	20 0	9 35	10 44	1 N	2 N
—	———	8	P 417	299 42	19 57	10 19	11 21	4 S	11 S
—	———	8	L 13	299 28	19 40	10 20	11 18	12 N	5 N
—	———	8	L 13	299 34	19 59	10 21	11 23	3 S	11 S
—	———	8	L 13	300 53	19 32	13 57	14 48	5 S	13 S
17	Tauri	7	L 8	55 35	22 34 N	12 17	13 23	10 N	4 S
21	———	7·8	L 11	59 58	23 1	13 59	14 50	2 N	6 S
—	———	7	L 13	60 30	23 2	14 55	15 40	5 S	11 S
22	———	6	P 295	73 59	23 59	12 46	12 56	4 N	2 N
23	Gemin.	8	Z 393	91 33	23 40	15 4	15 38	11 N	11 N
—	10 ———	7·8	P 51	91 41	23 40	15 11	15 45	11 N	11 N
—	11 ———	7	P 52	91 47	23 32	15 15	16 0	3 N	3 N
—	12 ———	8	P 53	91 48	23 20	15 20	15 57	9 S	9 S
—	———	8	Z 397	91 59	23 32	15 32	16 18	3 N	3 N
				AUGUST.					
				° ′	° ′	h m	h m	′	′
3	σ Scorpii	4	P 50	242 16	25 6 S	9 37	10 9	14 S	13 S
6	Sagitt.	8·9	P 253	282 19	23 30	7 20	8 18	11 N	7 N
—	———	8	L 13	283 20	23 9	10 47	cont.		
7	———	8	L 13	297 4	20 25	12 55	13 55	0	10 S
9	Aquarii	9	P 143	319 39	12 57	9 50	10 42	14 N	6 N
10	———	8·9	P 68	332 44	7 15	15 26	16 31	13 N	0
—	———	6·7	L 8	332 36	7 18	15 27	16 33	12 N	2 S
—	———	8·9	P 71	332 50	7 11	15 38	16 48	12 N	2 S
11	———	8·9	P 269	342 23	3 30	11 11	11 57	6 S	15 S
12	16 Piscium	6	P 132	351 33	1 0 N	7 1	7 57	4 N	7 S
—	19 ———	6	P 182	354 3	2 23	14 5	15 9	13 S	1 S
13	45 ———	6	P 65	3 51	6 35	10 34	11 19	13 N	3 N
14	———	7·8	L 11	16 53	12 1	14 58	16 16	5 N	9 S
16	Arietis	7	L 8	40 36	19 42	13 5	cont.		
—	———	7	L 8	40 44	19 31	12 43	13 43	8 N	2 S
—	47 ———	6	P 218	41 40	19 51	14 31	15 35	9 S	2 S
17	Tauri	7·8	L 11	54 55	22 3	14 12	cont.		
—	———	7	L 8	55 35	22 34	15 19	16 25	6 N	1 S
—	33 ———	6·7	P 199	56 18	22 35	16 35	17 39	4 S	10 S
18	———	7·8	L 13	67 21	23 37	10 44	11 23	9 N	5 N
—	140 ———	7	P 162	67 47	23 42	11 11	11 51	10 N	5 N
30	Scorpii	7	L 12	237 34	24 25 S	8 44	cont.		

[To be concluded in our next Number.]

LVII. *An easy Method of reducing Sidereal into Mean Time. By Dr.* T. L. Tiarks.*

THE following tables are useful to those who, having clocks or time-keepers that show mean time, have frequently occasion to calculate the time of transit of stars over the meridian, for reducing observations taken out of the meridian, or for ascertaining time by the altitude of a star, &c.

The right ascension of the star is to be taken from Table III.; and then, by applying the variation and periodical equations, to be reduced to the day of observation. From this right ascension thus reduced a number is to be subtracted, which is found by Tables I. and II., the quantity for the next preceding day in Table I. being increased by the quantity corresponding to the number of additional days in Table II., and this sum is then $\left\{\begin{matrix}\text{increased}\\ \text{diminished}\end{matrix}\right\}$ by the mean motion of the sun in right ascension for the longitude of the place of observation $\left\{\begin{matrix}\text{west}\\ \text{east}\end{matrix}\right\}$ of Greenwich.

If this number exceeds the right ascension, $23^h\ 56'\ 4''{\cdot}09$ is to be added to the latter, or the number is to be calculated for the next day, and the right ascension to be increased by 24 hours.

Table I.

		h	′	″			h	′	″
1823.									
Dec.	31	18	33	23·1	July	10	7	12	13·6
1824.									
Jan.	10	19	12	42·1		20	7	51	32·7
	20	19	52	1·2		30	7	30	51·8
	30	20	31	20·3		31	8	34	47·7
	31	20	35	16·2	August	10	9	14	6·8
Feb.	10	21	14	35·3		20	9	53	25·9
	20	21	53	54·4		30	10	32	45·0
	29	22	29	17·6		31	10	36	40·9
March	10	23	8	36·7	Sept.	10	11	16	80·0
	20	23	47	55·8		20	11	55	19·1
	23	0	3	39·4		30	12	34	38·1
	30	0	31	10·7	Oct.	10	13	13	57·2
	31	0	35	6·7		20	13	53	16·3
April	10	1	14	25·7		30	14	32	35·4
	20	1	53	44·8		31	14	36	31·3
	30	2	33	3·9	Nov.	10	15	15	50·4
May	10	3	12	23·0		20	15	55	9·5
	20	3	51	42·1		30	16	34	28·6
	30	4	31	1·2	Dec.	10	17	13	47·7
	31	4	34	57·2		20	17	53	6·8
June	10	5	14	16·3		30	18	32	25·9
	10	5	53	35·4		31	18	36	21·8
	30	6	32	54·5					

* Commuuicated by the Author.

TABLE

TABLE II.

Days.	′	″
1	3	55·91
2	7	51·82
3	11	47·73
4	15	43·64
5	19	39·55
6	23	35·46
7	27	31·37
8	31	27·28
9	35	23·19

TABLE III.

Æ of Stars in Mean Time for January 1, 1824.

	h	′	″		h	′	″
γ Pegasi	0	4	10·44	Arcturus	14	5	19·47
a Cassiop.	0	30	29·44	1 α Libræ	14	38	33·85
Polaris	0	57	51·86	2 α Libræ	14	38	45·31
α Arietis	1	56	57·19	β Urs. Min.	14	48	53·10
α Ceti	2	52	37·02	α Cor. Bor.	15	24	42·53
α Persei	3	4	17·08	a Serpentis.	15	33	3·20
Aldebaran	4	25	6·46	Antares	16	15	57·52
Capella	5	2	52·49	α Herculis	17	3	49·54
Rigel	5	5	14·99	α Ophiuchi	17	23	54·79
β Tauri	5	14	18·89	γ Draconis	17	47	35·73
α Orionis	5	44	42·27	α Lyræ	18	27	57·02
Sirius	6	36	18·24	γ Aquilæ	19	34	40·74
Castor	7	22	8·86	α Aquilæ	19	38	58·15
Procyon	7	28	51·56	β Aquilæ	19	43	25·80
Pollux	7	33	17·79	1 λ Capric.	20	4	35·34
α Hydræ	9	17	24·83	2 λ Capric.	20	4	59·08
Regulus	9	57	21·42	α Cygni	20	32	3·87
a Urs. Maj.	10	51	10·39	α Cephei	21	10	53·60
β Leonis	11	38	10·10	β Cephei	21	22	50·48
β Virginis	11	39	36·96	α Aquarii	21	53	8·95
γ Urs. Maj.	11	42	36·39	Fomalhaut	22	44	10·26
Spica Virg.	10	13	45·68	α Pegasi	22	52	14·70
η Urs. Maj.	13	38	21·49	α Androm.	23	55	22·92

Example.—Required the time of transit of α Aquilæ over the meridian of long. 4^h 56′ west, on the 3d June, 1824.

α Aquilæ, June 1, 1824		19^h	38′	58″·15
Reduction to June 4			+	3 ·22
α Aquilæ, June 3		19	39	1 ·37
N. for May 31	4^h 34′ 57″·2			
For three days	+ 11 47 ·73			
For 4^h 56′ W.	+ 48 ·49			
		4	47	33 ·42
Time of transit in mean time		14	51	27 ·95

T. L. TIARKS.

NEW EXPERIMENTS OF M. DŒBEREINER.

In the course of the last month some new and most interesting chemical experiments by Professor Dœbereiner of Jena have engaged the attention of the scientific world. These were first announced in this country in the short notice which we extract from the Quarterly Journal, in which Mr. Faraday states that he had verified these experiments. The last number of the *Annales de Chimie* which has reached us, also contains a note on the subject by MM. Dulong and Thenard, read to the Academy of Sciences on the 15th of last month, a translation of which we subjoin. The phænomena discovered by M. Dœbereiner were known to those gentlemen only by a paragraph in the *Journal des Debats;* they have however been earnestly engaged in researches on the subject, which will be read with great interest. To these we are enabled to add a paper read before the Bristol Philosophical Society by W. Herapath, Esq., for which we are obliged to the author and to that new institution.

Since these were prepared for the press, we have been so fortunate as to obtain Professor Dœbereiner's own account of his experiments, and we are very happy that it has arrived just in time for insertion in the present number.

"A most extraordinary experiment has been made by M. Döbereiner. It was communicated to me by M. Hatchette, and having verified it, I think every chemist will be glad to hear its nature. It consists in passing a stream of hydrogen against the finely-divided platina obtained by heating the muriate of ammonia and platina. In consequence of the contact, the hydrogen inflames. Even when the hydrogen does not inflame, it ignites the platina in places; and I find that when the hydrogen is passed over the platinum in a tube, no air being admitted, still the platinum heats in the same manner. What the change can be in these circumstances, M. Döbereiner has, no doubt, fully investigated; and the scientific world will be anxious to hear his account of this remarkable experiment, and the consequences it leads to.—M. F."—*Quarterly Journal of Science*, No. XXXI.

LVIII. *Note on the Property which some Metals possess of facilitating the Combination of Elastic Fluids**. *By* MM. Dulong *and* Thenard.—[*Read at the Academy of Sciences, the* 15th *Sept.* 1823.] Annales de Chimie, vol. xxiii. p. 440.

M. DŒBEREINER, professor at the university of Jena, has just discovered one of the most curious phænomena which

* Since the printing of this note, the writers have ascertained, 1st, that palladium in a spongy mass will inflame hydrogen, as platinum does; 2ndly, that

which the physical sciences can present. We are acquainted with the researches which he has made on this subject, only by the announcement which appeared in the *Journal des Debats* of the 24th of August last, and which is scarcely adapted to give an exact idea of it; and by a letter from M. Kastner to Dr. Liebig, which this gentleman, now at Paris, has had the goodness to communicate to us. It thence appears that M. Dœbereiner has observed that platinum in the spongy state causes, at the ordinary temperature, the combination of hydrogen with oxygen, and that the development of heat resulting from this action renders the metal incandescent. We hastened to verify a fact so surprising. We have found it very exact; and as the experiment can be made with the greatest ease, we are about to perform it in the presence of the Academy *.

Having no knowledge of the researches which the author of this beautiful experiment has no doubt undertaken in order to discover its theory, we could not resist the desire of ourselves making some attempts directed towards this object; and although we have not yet attained it, we think that the results of the observations which we have hitherto made, are not unworthy of the attention of the Academy.

In the experiment which we have been making, the spongy platinum becomes incandescent at the time when it is placed at the spot where the hydrogen which issues from the reservoir is become intimately mixed with the air. From this it was evident, that a small quantity of this platinum being plunged in a mixture of two parts of hydrogen and one part of oxygen, there ought to be a detonation; which the experiment confirmed. If the proportions of the gaseous mixture deviate much from those of water, or if there be present a gas foreign to the combination, as, for example, azote, the combination

that iridium under this form becomes very hot while it produces water; 3rdly, that cobalt and nickel in mass cause at about 300° the union of hydrogen and oxygen; 4thly, that platinum in the spongy state, when cold, formed water and ammonia with nitrous gas and hydrogen, and also acted on a mixture of hydrogen and protoxide of azote.

* The hydrogen gas lamp improved by M. Gay-Lussac is very convenient for making this experiment. The electrophorus is raised, or the conductors merely are detached; a very slight morsel of platinum in the spongy state is placed at the distance of about two centimetres from the opening by which the gas escapes, and as the cock is turned the stream of hydrogen gas falls mixed with air on the surface of the platinum. This becomes forthwith incandescent, and the hydrogen gas, once inflamed, keeps burning as it flows out, as if it had been lit by the spark.

In default of a lamp, the common apparatus may be employed which is used in laboratories for obtaining hydrogen gas. It is only necessary to take care that the gas be let out by a very small opening, in order that it may mix more completely with the air.

 goes

goes on slowly, the temperature rises little, and water soon appears condensing on the vessel. Platinum in the spongy state strongly calcined, loses the property of becoming incandescent; but in this case, it causes the combination of the two gases slowly and without a very sensible raising of the temperature. The finely-divided platina, obtained by a well-known chemical process, has no action, not even the slowest, at the ordinary temperature. The result with wires or laminæ is the same. The comparison of these observations might give rise to the idea that the porousness of the metal was an essential condition of the phænomenon; but the following facts destroy this conjecture.

We caused some platinum to be reduced into leaves as thin as the malleability of this metal admits of. In this state the platinum acts, at the ordinary temperature, on the mixture of hydrogen and oxygen, and with a rapidity proportioned to the tenuity of the leaf. We obtained some that caused detonation after some moments. But what renders this action still more extraordinary, is the physical state indispensable for its development. A very thin leaf of platinum, rolled round a cylinder of glass or suspended freely in a detonating mixture, produced no sensible effect at the end of several days. The same leaf crumpled like the wadding of a gun, acted instantly, and made the mixture detonate.

Leaves prepared as we have just mentioned, and which are then without effect at an ordinary temperature, wires, powder, and thick plates of platinum, whose action is always null, in the same circumstance, act slowly and without producing explosion at a temperature of from 400° to 572° F. according to their thickness.

We have observed that some other metals possess the same property as platinum. The very remarkable fact which Sir H. Davy discovered in the course of his researches on the safety-lamp, namely, that wires of platinum and palladium heated to a dull red become incandescent when plunged in a detonating mixture, having appeared to us referable to the same cause with the phænomenon in question, we were immediately led to try palladium.

The piece which we employed had been given to one of us by Dr. Wollaston; it must have been free from alloy; we were not able, however, to obtain very thin leaves from it; it shattered under the hammer of the beater. To this circumstance we attribute its inaction at the temperature of the atmosphere: however, it acts at least as well as platinum, of the same thickness, at an elevated temperature. Rhodium, being brittle, could not be subjected to the same operation; but

but it caused the formation of water at a temperature of about 464° F.

Gold and silver in thin leaves act only at high temperatures, but always under that of the ebullition of mercury. Silver is less efficacious than gold. A thick lamina of the latter does indeed act, though with more difficulty than the leaves; and a thick lamina of silver has an action so weak as to be doubtful.

We have also tried if other combinations could be effected by the same means. Carbonic oxide and oxygen combine, and nitrous gas is decomposed by hydrogen at the common temperature by contact with platinum in the spongy state. The fine leaves of the same metal do not produce the combustion of the first-mentioned gas, except at a temperature above 572° F. Gold leaf causes it also at a degree near the boiling of mercury.

Finally, olefiant gas mixed with a suitable quantity of oxygen is completely transformed into water and carbonic acid by platinum in the spongy state, but only at a temperature of more than 572° F.

We would call to mind, on the subject of the preceding experiments, that one of us showed long since that iron, copper, gold, silver, and platinum, had the property of decomposing ammonia at a certain temperature, without absorbing either of the principles of that alkali; and that this property appeared inexhaustible. Iron possesses it in a higher degree than copper, and copper more than silver, gold, and platinum, in proportion to the surfaces.

Ten grammes of iron wire are sufficient for decomposing, within a few hundredths, a current of ammoniacal gas rather rapid, and kept up for eight or ten hours, without the temperature passing the limit at which the ammonia completely resists decomposition. Thrice that quantity of platinum wire of the same thickness, does not produce nearly a like effect even at a higher temperature.

The remarkable results of this experiment depend, perhaps, on the same causes as those which make gold and silver effect the combination of hydrogen and oxygen at 572° F., solid platinum at 518° F., and spongy platinum at the ordinary temperature.

Now, if we observe that iron, which so well decomposes ammonia, does not effect, or but with difficulty, the combination of hydrogen with oxygen, and that platinum, which is so effective for this latter combination, produces but with difficulty the decomposition of ammonia, we are led to believe that among the gases some have a tendency to unite under the influence of the metals, while others have a tendency to separate;

separate; and that this property varies according to the nature of each. Those of the metals which would best produce the one effect would not produce the other, or but in a less degree.

We shall abstain from offering some further conjectures to which these singular phænomena give rise in our minds, until we shall have terminated the experiments which we have undertaken in order to verify them.

LIX. *On Dœbereiner's new Experiment.* *By* William Herapath, *Esq.**

October 20, 1823.

THE philosophical publications and newspapers have lately announced an experiment by Dœbereiner with hydrogen gas and platinum in a finely divided state. Those announcements were either not accompanied by a description, or by one evidently so inaccurate as to cause chemists in general to treat it as an attempt at the marvellous: the impression made upon my mind was, that Dœbereiner himself had not discovered the changes which were effected; and I regret that Mr. Faraday of the Royal Institution did not prosecute his inquiries further than verifying the one experiment.

I therefore made a series of experiments, to make myself acquainted with the phænomena; and, as they throw some light upon the subject, I beg to offer them to this Society for its information: but I shall first premise, that it was before known that oxygen, iodine, chlorine, and sulphur, would, in some cases, where they rapidly united, give out caloric and light sufficient to produce those effects which have been termed ignition and combustion: but no such knowledge had been acquired of hydrogen. Consequently, as this experiment of Dœbereiner's appeared to prove that pure hydrogen also had that property, it was of importance that it should be minutely investigated.

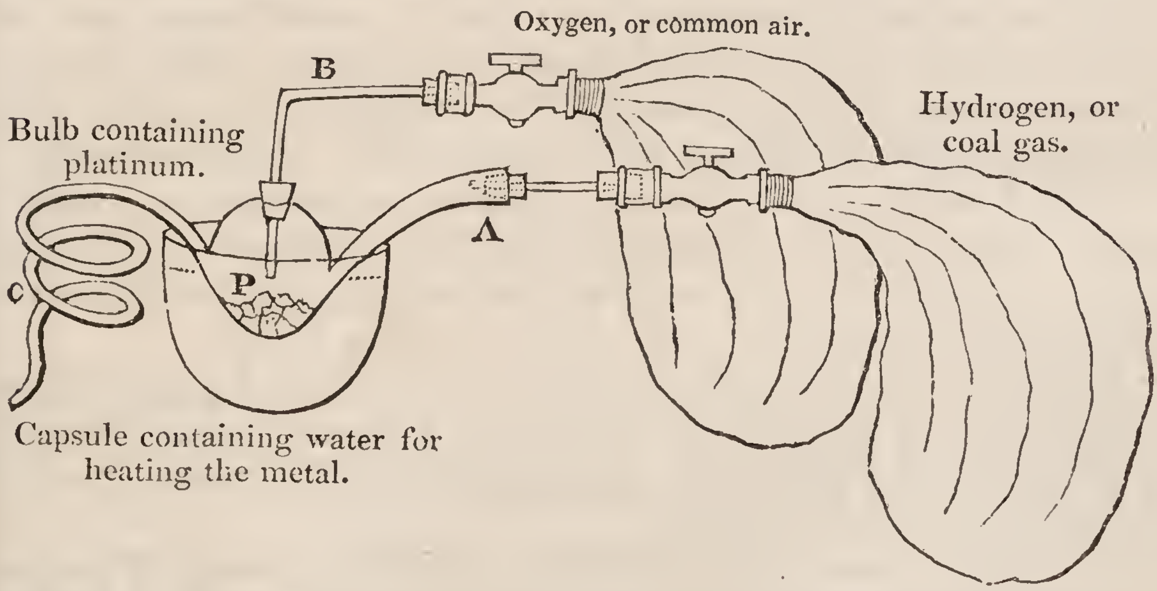
Exp. 1. A stream of hydrogen as generated from zinc, &c., and therefore mixed with common air, was passed upon 5gr. of the spongy mass (as obtained by heating to redness the ammonia-muriate of platinum) in a thin glass capsule; the metal became red hot quite close to the orifice from which the stream issued; but as the gas became purer from the smaller proportion of air, I found it necessary to remove the metal to a little distance; the great heat set fire to the hydrogen; I extinguished it, and occasionally removed the platinum so as to prevent the recurrence of flame. After the experiment had continued half an hour, the metal was examined; there was no

* Read before the Bristol Philosophical Society of Inquirers; and communicated by the Author.

change

change in its appearance, nor had it increased or diminished in weight. The phænomenon then, I presume, was not occasioned by any change the metal had undergone. As it was necessary to remove the platinum to a greater distance as the gas became purer, it appeared to indicate that the presence of atmospheric air or oxygen was as essential as the hydrogen: to prove the truth of this idea, a large thin bulb having a hole in its side was blown at the end of a piece of tube; into which tube hydrogen was conveyed. The platinum was in an atmosphere of hydrogen, but it did not become hot; a fine tube was then introduced into the hole in the bulb, and a stream of common air made to act on the metal; it immediately glowed, and continued to do so as long as both currents were directed towards it, while water was found on the sides of the bulb, which increased as the experiment went on.

Exp. 2. It seemed to result from this, that the platinum has the curious property of causing oxygen and hydrogen to combine at a low heat: I therefore directed a stream of the mixed gases from the gas blowpipe upon a portion of the spongy mass, when it glowed as before and ignited the gases at the jet pipe; but I found that the metal required to be heated a little before the phænomenon occurred: as this was not necessary when hydrogen was admitted direct from the retort, the temperature of the atmosphere being sufficient, it favoured the idea that a small quantity of caloric was requisite, which quantity was carried over from the retort, but which it afterwards lost in the cool vessel. I therefore constructed a small apparatus with which I could repeat the experiments under more favourable circumstances.



Exp. 3. The apparatus was kept full of hydrogen from the tube A; the metal did not glow at any temperature, and the gas was inflammable as it escaped from the capillary orifice of the tube

tube C. A stream of common air was made to act on the metal through the perpendicular tube B; it now glowed and continued to do so as long as *both* streams were continued; when either failed, it immediately cooled; while the metal continued red hot, there was no inflammable gas issuing from the tube C, while water rapidly condensed within it.

In these experiments, the bulb containing the platinum was placed in water at the respective temperatures of 60°, 70°, 80°, 90°, 100°, and 98° (blood heat); at the first four no effect was observable, while it was at the last two. After 130 cubic inches of hydrogen had passed, the water in the tubes was driven off by heat, and the apparatus weighed; it had lost ·08gr., which I attributed to accident; but to prove it, I passed 130 cubic inches more, and upon reweighing there was no further diminution.

Exp. 4. The bladders were now changed so that the platinum was in an atmosphere of common air; there was no action at any temperature; but as soon as hydrogen was passed down the tube B, and the platinum was heated to 98° or 100°, the gases were condensed as before.

Exp. 5. Coal gas mixed with common air was next passed through the tube B (A being stopped); it caused the metal to glow, but not until the temperature was much increased. I did not ascertain the exact point, as I consider that it would vary with different proportions.

Exp. 6. The tube B was then connected with the gas blowpipe and a fine stream admitted, taking care to avoid explosion or inflammation; at 100° it glowed as before. If in either of the foregoing cases any moisture was present upon the metal, no action took place; whereas the combination was effected more readily and at a lower temperature, when the experiment was repeated a second time within a short time after the first, which I suppose was owing to the platinum being a very bad conductor of caloric, and consequently not cooling to the temperature of the surrounding medium within that time. I found that the same platinum might be used for any length of time, with the precaution of using it dry.

Imagining that the effect might be electrical, I placed some of the metal in a platinum foil cup on Bennett's electrometer; it glowed in the parts not adjoining the foil, but no signs of electricity were observable.

To try if it was owing to the nonconducting power, I passed a stream of the gases upon asbestus, which is a nonconductor of caloric and finely divided (but fibrous instead of spongy); but there was no action.

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I tried other finely divided metals, such as lead as precipitated by zinc, and gold and silver as thrown down by copper; but without success.

From those experiments I am perhaps warranted in concluding,

1st, That no chemical change takes place in the platinum, and therefore I presume its effect to be mechanical:

2nd, That a change does take place in the condition of the gases, which change is their union to form water:

3d, That in case the gases have the temperature of 55°, the platinum requires a temperature of 98° to cause them to unite:

4th, That as the condensation of the gases is the only change in the substances used, we must infer that the greatly increased heat of the platinum arises from that condensation.

I have here pointed out the proximate cause of the heat of the platinum, but the ultimate I have not been able to discover. It is therefore left as a problem to future inquirers, Why platinum in a state of minute division should cause the union of oxygen and hydrogen at 100°, whereas their lowest combining temperature without it is 700°?

If the effect of the metal be mechanical, I have no doubt that other substances will be found having the same power, although I have not succeeded in selecting them.

The phænomena altogether are singular, and appear intimately connected with aphlogistic phænomena, or at least to stand in the same relation to them as they do to rapid combustion: for instance;

At 100°, spongy platinum causes oxygen and hydrogen to combine.

At 700°, they unite without it silently.

At 800°, explosion attends their combination.

At red heat (about 1000°), platinum-, silver- or brass-wire causes carbon, hydrogen, and oxygen to combine, forming water, acetic acid, and resin.

At a white heat, carbon, hydrogen, and oxygen, combine, forming water and carbonic acid.

William Herapath.

LX. *On some newly discovered remarkable Properties of the Protoxide, Oxidized Sulphuret, and Metallic Powder of Platinum. By Professor* Dœbereiner*.

I HAVE already proved that the protoxide of platinum obtained by Edmund Davy's method, has the property of

* From Schweigger and Meinecke's *Neues Journal für Chemie, &c.* N. R. band viii. p. 321.

causing alcohol, placed in contact with it, to attract oxygen gas, and to become converted into acetic acid and water; and that this property is likewise possessed by the oxidized sulphuret of platinum, prepared by treating a solution of that metal with sulphuretted hydrogen, and exposing in a dry state the sulphuret formed by that means, to the action of atmospheric air for some weeks. In this very remarkable process, 1 atom (=46) of alcohol combines with 4 atoms ($=4\times8=32$) of oxygen, and forms with it 1 atom (=51) of acetic acid, and 3 atoms ($=3\times9=27$) of water; that is to say, equal volumes of the vapour of alcohol and oxygen gas, become equal volumes of acetic acid and aqueous vapour; for 1 atom of water is requisite to the isolated existence of acetic acid. The respective proportions in which acetic acid and water appear in this case, are exactly the same as those which they bear to each other in crystallized sugar of lead, and also in the subacetate of copper; the quantity of water in acetate of soda is exactly double that which is contained in each of the former acetates.

After having finished my experiments on this process of the formation of acetic acid, I took the opportunity of ascertaining the relations of the two above-named preparations of platinum to different elastic fluids. The results of the experiments instituted for that purpose are interesting; for I found,

1. That neither oxygen nor carbonic acid gas was absorbed by the protoxide, or by the oxidized sulphuret of platinum; but that those substances absorbed every inflammable gas.

2. That 100 grains of protoxide of platinum absorb from 15 to 20 cubic inches of hydrogen gas, during which absorption so much caloric is evolved, that the protoxide becomes ignited, and the hydrogen burns with detonation, if it had been previously mixed with oxygen or with atmospheric air.

The preparation of platinum, charged with hydrogen, has the property of greedily attracting as much oxygen gas as is requisite for the saturation of the hydrogen it contains. If atmospheric air, therefore, be suffered to enter the tube containing it, it instantly deprives it of its oxygen, and even forms ammonia with a portion of the residual nitrogen, if there be not sufficient oxygen present for its saturation. By this agency the oxide of platinum is reduced, and thereby loses its remarkable property of disposing alcohol to become acetic acid, and also that of condensing hydrogen gas; but, what is very remarkable, it retains the property of determining the latter substance to the state in which it combines with oxygen gas, and becomes water; and so much heat is evolved during this combination, that if the hydrogen gas be mixed with pure

oxygen,

oxygen, and the volume of the mixture be rather large, the platinum becomes red-hot. I could not but conclude, from this most remarkable phænomenon, that the finely-divided metallic platinum which is produced by the igneous decomposition of the ammonia-muriate, would perhaps exhibit this singular effect upon the detonating mixture; and, to my great satisfaction, this supposition was confirmed by the experiment. Some platinum powder, prepared from the saline precipitate just named, was wrapped up in white blotting-paper, and brought into contact with the hydrogen gas; and, as might be expected, no absorption took place, nor any other perceptible mutual action. Upon this I caused atmospheric air to have access to the platinum powder in contact with the hydrogen, and after the lapse of a few moments that remarkable reaction took place; viz. the gas diminished in volume; and in ten minutes all the oxygen of the atmospheric air admitted had condensed with the hydrogen into water. I afterwards mixed pure oxygen gas with the hydrogen in contact with the platinum; a condensation of both immediately took place, and the platinum heated to such a degree, that the paper in which it was wrapped was suddenly charred. These experiments were repeated about thirty times on the same day, July 27, 1823, on which I discovered this remarkable phænomenon, and with the same success every time.

What useful applications of this discovery may be made in oxymetry, the synthesis of water, &c., I shall hereafter state more circumstantially. I shall at present merely observe, in conclusion, that the entire phænomenon must be considered as an electric one, that the hydrogen and platinum form a voltaic combination, in which the former represents the zinc;—the first established instance of an electric alternation formed by an elastic fluid and a solid substance; the application of which will lead to further discoveries.

I obtained another interesting result in an experiment on the relation of the oxidized suphuret of platinum to carbonic oxide. I found that this gas is always diminished to half its bulk when it comes into contact with the sulphuret, and that the remaining gas is not carbonic oxide, but carbonic acid. *The carbonic oxide gas is therefore decarbonized by the oxidized sulphuret of platinum, and thereby changed into carbonic acid.*

SUPPLEMENT*.

I send you a short supplement to the paper communicated to you some days ago, on the newly discovered properties of several

* From a letter of Professor Dœbereiner to Professor Schweigger, dated Jena, Aug. 3, 1823.

several preparations of platinum. That the continuation of the experiments on this interesting subject would lead to new discoveries, was to be expected. I merely mention to-day, that I have succeeded in making the observed dynamic relation of the platinum powder to the hydrogen gas, appear in a very splendid manner by experiment. If hydrogen gas be suffered to issue from a gasometer through a capillary tube bent downwards, upon the platinum contained in a small glass funnel sealed at the bottom, so that the stream may mix with the atmospheric air before it comes in contact with the platinum, which is effected when the tube is from 1 to $1\frac{1}{2}$ or 2 inches distant from the platinum, the latter almost instantly becomes red- and white-hot, and remains so, as long as the hydrogen continues to flow upon it. If the stream of gas be strong, it becomes inflamed, particularly if it has already been mixed in the reservoir with some atmospheric air. This experiment is very surprising, and astonishes every beholder, when he is informed, that it is the result of the dynamic reaction of two species of matter, one of which is the lightest and the other the most ponderous of all known bodies. That I have already applied this new discovery to the formation of a new apparatus for procuring fire, and of a new lamp; and that I shall avail myself of it for much more important purposes, you may well suppose beforehand:—more of it in my next.

Dœbereiner.

LXI. *On the Parallax of α Lyræ. By* John Pond, *Esq. Astronomer Royal, F.R.S.**

My former experiments with a fixed telescope upon α Cygni have always appeared to me so decisive, as to render hopeless any further attempt to discover its parallax; but respecting that of α Lyræ, my observations with the mural circle were not equally satisfactory; for among the observations of this star we may find occasional discordances that admit of being interpreted in favour of parallax. And although I have been inclined myself to attribute these irregularities to other causes, yet their existence made it desirable to institute new experiments. The method with a fixed telescope, which I had contrived for α Cygni, could not here, I found, be applied successfully; there being no star of nearly the same altitude but opposite in right ascension sufficiently bright to be observed throughout the year, a circumstance quite essential to that mode of observation. I have employed therefore the mural circle to investigate, 1st, the difference of parallax be-

* From the Philosophical Transactions for 1823, Part I.

tween

tween γ Draconis and α Lyræ: 2dly, the absolute parallax of the latter star; the Dublin observations indicating, it may be remembered, that the parallax of γ Draconis is insensible, but that of α Lyræ a very perceptible quantity. The processes employed in these two investigations being very different, I shall consider each of them separately.

On the Difference of Parallax between γ Draconis and α Lyræ.

It is impossible to conceive a more simple process than that of determining with the mural circle the difference of polar distance between these stars. From their proximity in right ascension, the operation is the same as that of measuring the angular distance of two terrestrial objects, about 12° asunder, with a theodolite surrounded by six microscopes: for the mural circle, in principle, exactly resembles a vertical theodolite; with this difference, that its microscopes, instead of being placed on a frame-work of brass, are securely fixed on a stone-pier. Now I find that the angular distance thus measured in winter does not differ one-tenth of a second from the same angular distance measured in summer; and therefore, that the difference of parallax between the two stars is absolutely a quantity too small to be measured. In this investigation, it is to be considered that any constant error in the determination of the absolute polar distances has nothing to do with the question, it being the difference only of those distances at opposite seasons that is required. To render all errors throughout the whole course of observation as constant as possible, the telescope remained fixed to the same part of the limb of the instrument, and the utmost pains were taken to reduce the temperature in the Observatory to that of the outer air; the difference throughout the year not exceeding one degree. The winter of 1821-1822 was extremely favourable for astronomical observation; there was an unusual number of fine nights, and the weather was so mild and uniform, that we were enabled to equalize the temperature, so as to make it of no importance whether the observations were computed by the outer or inner thermometer; and it is to this circumstance, in a great measure, that I attribute the perfect coincidence between the observations at different seasons.

It has been objected, however, that perhaps some unexpected effect of temperature deranges the instrument by the exact quantity of the difference of parallax attributed to these stars by Dr. Brinkley; if we suppose a derangement from temperature so considerable as to give a sensible error, even after being diminished by the effect of six microscopes, we should expect the error to be much greater when the experiment

periment is tried with two microscopes only; for to suppose the contrary, would be to deny the tendency of six microscopes to correct the errors of two. Now I find the same difference of polar distance whether I employ two microscopes or six; temperature, therefore, cannot materially have vitiated the results by causing derangement in the form of the instrument.

In the whole of the above process I do not see one objectionable point, and if called upon to invent an instrument for this particular experiment, I could not devise one more perfect in principle than the mural circle.

Whoever will compare the above simple process with the more complicated one necessarily employed in using an instrument with two microscopes, turning freely in azimuth, will not hesitate, I think, in deciding upon which of the two instruments temperature is likely to produce the greatest error.

On the absolute Parallax of α Lyræ.

The preceding observations only indicate that γ Draconis and α Lyræ have the same parallax, or that their difference of parallax is zero; but they have no tendency to show what is the actual magnitude of the parallax that the two stars have in common. If indeed we admit it to be proved, by the observations of Bradley, and the more recent ones of Dr. Brinkley, that the parallax of γ Draconis is insensible, we may then infer from the observed difference what is the parallax of the other star. But the method of investigation that we are now about to consider, does not depend on such an admission.

Having successfully adopted the method of observing by reflection, I was desirous of employing it in a series of observations upon α Lyræ, with a view to determine this question. This series began on the 1st of July 1822, and has been continued to the present time*. Although this period embraces only half the interval in which the greatest change or double parallax is affected, a circumstance which at first may appear very disadvantageous, yet that is more than compensated, in my opinion, by the number of observations, and by a uniformity of temperature, such as never can be expected in the extreme seasons of winter and summer.

In observations of this nature the effects of temperature upon the instrument itself, and the uncertain refractions of the ray of light when brought into the lower part of the room, may produce errors of no inconsiderable magnitude, with reference to a question of so much nicety as the present.

* Since the date of this paper (read Nov. 14, 1822) the observations have been continued throughout the winter, and the results will be found in the Table, Phil. Trans. p. 61.

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I can show however in the present as in the former process, that no error from temperature, affecting the instrument, has introduced itself into this series of observations; for I obtain the same result from the readings with two microscopes as from those made with six.

In the case of two microscopes, the angular distance is measured upon two arcs only. Now it cannot be for a moment contended that an error from temperature, so great as not to be corrected by six microscopes, will not be much exaggerated by employing only two. The errors then, if any, must arise from the effects of temperature on refraction, and not from the changes it occasions in the instrument. But from the season which I have chosen for this investigation, and from the care that has been taken to equalize the temperature, the errors arising from the latter cause must be almost insensible. My observations, thus conducted, indicate in the most decided manner, that the parallax of α Lyræ cannot exceed a very small fraction of a second. The advantages and disadvantages of the Dublin and Greenwich methods are in this process much more nearly balanced than in the former. The Dublin instrument has the great advantage of determining the zenith distance in the course of a few minutes; whereas at Greenwich twenty-four hours at least, and frequently several days elapse, before a complete observation of the double altitude can be obtained by the method of reflection. This disadvantage attending the Greenwich method could only be remedied by employing two mural circles for observing a star on the same night, both by direct vision and by reflection.

I have now to consider that argument on which the greatest reliance in favour of parallax has been placed, namely, that founded on the actual determination of the solar equation from the observations made with the Dublin instrument.

This argument may, I think, be thus stated. By a series of observations made with a given instrument two equations have been disengaged, previously considered as unknown in amount, but known only as to the law of their variation. Of these, one is much smaller than the other. Hence it is inferred, that as the instrument has faithfully disengaged the smaller equation (respecting which there is no dispute), it must be admitted with equal fidelity to have disengaged the larger, which might be supposed the easier operation of the two. This reasoning is strictly logical, as proving the disengagement of two equations; but it by no means proves the larger equation to be caused by parallax. The larger equation here to be disengaged is after all so small, that it is impossible, in different points of its period, to show that the law assumed coincides

coincides with observation; it is only a rude agreement at the points of the greatest and least variation that can be demonstrated. The disengagement of the larger equation only proves therefore the existence of some regularly recurring cause, acting with greatest effect at the extreme seasons.

The reason, I conceive, why Dr. Brinkley does not find parallax in γ Draconis is, that with respect to the zenith point, his instrument, like every one of a similar construction, is a perfect instrument. No portion of the arc is employed, nor can temperature here occasion any errors by its changes. As the star to be examined recedes from the zenith, the instrument becomes less and less perfect; and he finds a small parallax in α Cygni, a larger in α Lyræ, and oftentimes a still larger in stars more remote from the zenith. An additional reason for suspecting that the discordances observed arise from temperature is this: the greatest supposed parallax is found in those stars whose maximum and minimum of parallax would fall in the extreme seasons, and it is not at all improbable that irregular refraction, arising from the unequal state of the temperature within and without the Observatory, may have had a considerable share in occasioning the Dublin discordances, combined, perhaps, with the effect of the changes of temperature upon the instrument itself. It is a circumstance not hitherto sufficiently noticed by astronomers, that there are many cases where the smallest disturbing cause will produce an error quadruple of its own amount; and consequently, that the greatest error to which we are liable from such a cause at any one observation will be only one-fourth of the difference that we can detect between the most discordant of them. Of such a nature are those disturbances which, like refraction for instance, introduce errors, both positive and negative, into the determination of either extremity of the arc that measures the distance between two stars.

By a singular combination of circumstances, not probable certainly when considered *a priori*, but by no means impossible, the variation caused by change of temperature may follow an annual law so little differing from that of parallax, as to bring out the assumed parallax, and to leave the solar nutation disengaged.

Notwithstanding the importance of these investigations to the history of astronomy, and to our forming a correct notion of the system of the universe, yet our decision ultimately turns upon so very small a quantity, that our having reduced the inquiry to these narrow limits, rather tends to show the perfection of each instrument than the defect of either.

On former occasions I considered the question of parallax

in

in the particular case of α Lyræ as undecided, and as perfectly open to future investigation; but the observations of the present year have produced on my mind a conviction approaching to moral certainty. The history of annual parallax appears to me to be this: in proportion as instruments have been imperfect in their construction, they have misled observers into the belief of the existence of sensible parallax. This has happened in Italy to astronomers of the very first reputation. The Dublin instrument is superior to any of a similar construction on the continent; and accordingly, it shows a much less parallax than the Italian astronomers imagined they had detected. Conceiving that I have established, beyond a doubt, that the Greenwich instrument approaches still nearer to perfection, I can come to no other conclusion than that this is the reason why it discovers no parallax at all.

LXII. *On a new Steam-Engine Governor. By Mr.* J. Preuss, *of Hanover, Engineer, late Inspector-General of French Imperial Forests, Fellow of several learned Societies**.

IT has been observed by Mr. Doolittle of America, that the well-known centrifugal steam-engine governor, invented by the celebrated James Watt, is a less perfect regulator of velocity than might be wished for, particularly for purposes which require a great regularity and nicety in the motion of the steam-engine; as for instance, in cotton mills, &c. Indeed the centrifugal forces of two equal masses which perform their revolutions round a central point in equal times, being to each other as the radii of the described circles, it follows, that if the two balls revolved with an adequate speed, so that their centrifugal force, which tends to make them fly asunder, was exactly counterbalanced by their weight, which tends to make them collapse, they would continue in their places, but without exerting any pressure upon them.

Let us suppose now that their speed happened to increase by a quantity, however small; the balls would then fly out, and as long as the motion was carried on with the same speed, their centrifugal tendency would increase as the interval increased which separated them.

Let us further suppose them to move with an interval double of that which they keep when in their seats, and so as to make the same number of revolutions in a given time as they did when in their places,—then their tendency to fly out will be double,

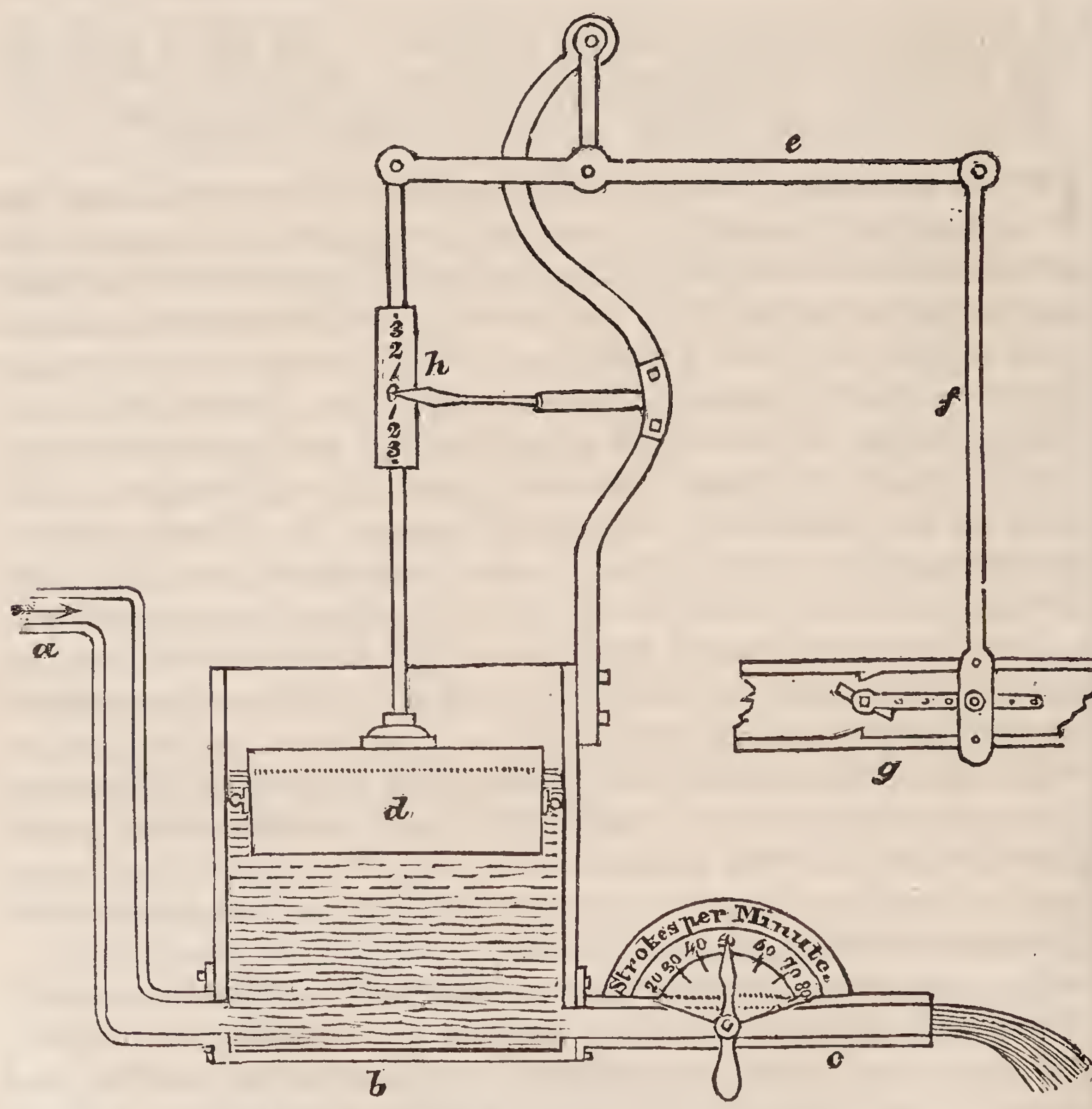
* Communicated by the Author.

double, and it will be necessary that their speed should be diminished by half, in order to restore the equilibrium between their centrifugal force and their weight, and a still more considerable decrease of speed would be requisite to make the balls collapse. Hence the speed must oscillate between the maximum and the minimum; while, in order to have an equal motion of the machine, the difference between the two possible extremes ought to be as small as can be.

I have endeavoured to invent a contrivance which might not be subject to the inconveniencies now stated: how far I have succeeded in my task, I leave to practical engineers to decide. The annexed figure shows a section of the apparatus.

Description.

a. Is a water-pipe connected to a small forcing-pump, which draws water, in order to supply the hot-water pan, out of which the boiler is fed. This water may either be drawn from the condensor or from a well.



b. Is a small cistern supplied by the said pump.

c. Pipe provided with a regulating cock, through which the water

water flows out of the tank into the hot-water pan. The cock can be adjusted upon the scale of the sector, so as to transmit the requisite quantity of water in a given time.

d. A float or close box of copper, or varnished sheet-iron, or tin, filled with atmospheric air.

e. Lever connected on one side to the rod at the top of the float, and at its opposite extremity to the rod *f.*

f. A rod attached to the small lever of the throttle valve *g.*

g. Throttle valve connected with the pipe which conveys the steam from the boiler to the steam cylinder. This valve being turned either up or down, increases or reduces the steam-passage and affects the speed with which the piston moves in the cylinder.

h. Index fixed against the support of the lever *e*, showing upon the scale attached to the float-rod, such variations as may occasionally take place.

After this, it is evident that if the water-pump which supplies the pipe *a* is constructed and placed so as to throw up an equal quantity of water at every stroke, the cock with its hand being turned upon such a figure of the index as will correspond with the desired number of strokes per minute, this cock will always deliver the same quantity of water in a given time; and though a greater or smaller portion may be pumped into the tank *b*, yet neither more nor less water can flow out of it (for the possible slight variation of pressure by the different heights of water in the tank is too trifling to deserve any consideration in the present instance). Now let us suppose the cock *c* were regulated upon 30 strokes per minute, and that the engine happened to make 32 strokes, which would certainly be so slight an increase of speed as hardly to be perceptible even in very delicate work: yet this small irregularity could not even continue for a minute; for at the end of that time there would be a surplus of water in the tank equal to the bulk of two pump strokes, which would raise the float *d* in a degree which would be the greater, the smaller the capacity of the tank had been made in proportion to the bulk of a pump stroke; and this elevation of the float *d* would act upon the throttle valve with more efficacy, the shorter the small lever *g* was pinned to the rod, and the longer the right-hand side of the main lever *e* was made in comparison with its left-hand side.

J. Preuss.

LXIII. *Notices respecting New Books.*

A History and Description of the French Museum of Natural History. 2 vols. 8vo, published at Paris, and sold by George Sowerby, 33 King Street, Covent Garden.

THIS work has just been published in Paris under the immediate auspices, and indeed by the direction, of the learned professors of that noble establishment. At a time when the singular management of the British Museum has been so completely brought under the public notice, our continental neighbours may well feel proud at the appearance of this work. It commences with a history of the *Jardin du Roi*, which we could wish that every person intrusted with the government of a National Museum would carefully peruse. The reader indeed will see that, notwithstanding the proud edifice France has now raised for the study of Natural History, the *Jardin du Roi* in its early days had many difficulties to contend with, owing to the trustees, superintendants, or governing persons, whatever may have been their titles, being men better acquainted with the intrigues of courts than with the beauties of nature. He will see, that while some who were indifferent to the science, or had affairs of greater importance to attend to, left the government of the infant institution to economical persons not only ignorant of Natural History, but who were jealous of its progress merely because they did not understand it, public property never failed to suffer, and great expense in the end to be incurred by the nation. He will not perhaps find magnificent bequests to the French nation to have been dispersed and sold in opposition to the manifest intentions of the generous and patriotic donors; still less will he find such sales to have taken place merely because the trustees of the National Museum knew little and cared less * about the value of what was placed under their protection. Although indeed in this respect the interest of the tale may be a little deficient, we may safely say that the naturalist will derive much amusement from watching the rise and progress of the *Jardin des Plantes*, the history of which is intimately interwoven with that of some of the most celebrated men of France, such as Tournefort, Jussieu, Buffon, Vicq d'Azir, Fourcroy, &c. The general reader also cannot but receive pleasure from the description of the contents of the Museum, which is interspersed with in-

* As a means of infusing a portion of science into the direction of the British Museum, we have heard it suggested that the Presidents of the Antiquarian, Linnæan, and Geological Societies, or some sufficient representatives of the great scientific bodies of the Metropolis, should be added to the number of Trustees.

teresting

teresting anecdotes, and is given in so popular and instructive a manner as to render any previous knowledge of Natural History quite unnecessary for its perusal.

The work however is certainly most valuable, as showing us what a National Institution for the study of Natural History may become under proper management; and we cannot refrain from expressing our gratitude to the editor M. Royer, who has thus provided us with an authentic description of the only public establishment of which we have reason to envy France the possession.

Two editions of the work have been printed, the one in French and the other in English, and both are ornamented with a number of excellent engravings of the romantic *Jardin du Roi.*

A Series of Lectures upon the Elements of Chemical Science, lately delivered at the Surry Institution; comprising the Basis of the new Theory of Crystallization, and Diagrams to illustrate the Elementary Combinations of Atoms, particular Theories of Electrical Influence, and of Flame; with a full Description of the Author's Blow-Pipe, and its Powers and Effects, when charged with certain Gases, &c. &c. with eight Plates. By Goldsworthy Gurney. In Octavo.

An Elementary Treatise on Algebra, Theoretical and Practical; with Improvements in some of the more difficult Parts of the Science, particularly in the general Demonstration of the Binomial Theorem, the Solution of Equations of the higher Orders, the Summation of Infinite Series, &c. Dedicated, with Permission, to Dr. Gregory, Professor of Mathematics in the Royal Military Academy. By J. R. Young, Teacher of the Mathematics, Navigation, Nautical Astronomy, &c. In Octavo.

Medico-Chirurgical Transactions. Vol. XII. Part II. 8vo, with several coloured Plates. Published by the Medical and Chirurgical Society of London.

Formularly for the Preparation and Mode of Employing several New Remedies; namely, the Nux Vomica, Morphine, Prussic Acid, Strychnin, Veratrine, the active Principles of Cinchonas, Emetine, Iodine, &c. with an Introduction, and copious Notes. By Charles Thomas Haden, Surgeon to the Chelsea and Brompton Dispensary, &c. Translated from the French of Magendie. In 12mo.

An Improved System of Arithmetic (in two parts), for the use of Schools and Counting-Houses. By Daniel Dowling, Master of the Mansion House Academy, Highgate; and Author of the Key to Dr. Hutton's Course of Mathematics. Part I. Second Edition.

Chemical

Chemical Recreations: a Series of amusing and instructive Experiments, which may be performed easily, safely, and at little Expense. To which are prefixed, First Lines of Chemistry; wherein the principal Facts of the Science, as stated by the most celebrated Experimentalists, are familiarly explained. With a minute Description of a cheap and simple Apparatus; illustrated by Seventy engraved Figures of the different Parts of it. In one vol. 18mo. 3*s.*

A Treatise on Subterraneous Surveying, and the Variation of the Magnetic Needle. By Thomas Fenwick, Colliery Viewer and Surveyor of Mines; Author of the Essays on Practical Mechanics. Second Edition.

Ueber den Antheil welchen der Erdboden an den Meteorischen Processen nimmt; On the Part which the Earth takes in Meteoric Processes: a discourse delivered at the anniversary of the Halle Society of Natural History, July 5, 1823, by the president Professor F. R. G. Meinecke. 8vo, 35 pages.

In this discourse the Author assigns the alternate absorption and giving out of air by the porous strata of the globe, as a main cause of the rise and fall of the barometer.

Observations Mineralogiques sur les Environs de Vienne; by Count Razumowski. 1822. 4to.

Ricerche sopra l'Intendimento del Cane e degli altri Bruti, &c. Researches on the Intellect of the Dog and other Animals; by F. Orioli, Pesaro and Bologna, 1823, 8vo.

Gaspari Georgii Caroli Reinwardt *Oratio, de Augmentis quæ Historiæ Naturali ex Indiæ Investigatione accesserunt, publice habita.* 3 Maii 1823. Leyden, 4to, 23 pages.

This is a discourse delivered at the commencement of his professorship by Dr. Reinwardt, who after a residence of some years in Java, devoted to scientific objects, has been appointed on his return to the chair of chemistry and natural history in the university of Leyden.

Dictionnaire classique d'Histoire Naturelle; by MM. Andonin, Brongniart, Decandolle, Edwards, Geoffroy St. Hilaire, Latreille, A. Richard, Bory de St. Vincent, &c. vol. iii. from CAD to CHL, 8vo, pp. 592.

Mémoire sur la Distribution Geographique des Animaux Vertébrés, moins les Oiseaux; by M. Desmoulins.

Chimie appliquée à l'Agriculture; by Count Chaptal. 1823, 2 vols. 8vo.

Recherches Balistiques sur les Vitesses Initiales, le Recul et la Resistance de l'Air; by L. M. Prosper Coste. 1823. 8vo.

ANALYSIS OF PERIODICAL WORKS ON BOTANY.

Curtis's Botanical Magazine. No. 441.

Pl. 2433, *Phaylopsis longifolia*, "caulibus erectis, foliis oblongo-ovatis acuminatis reflexis, spicis axillaribus brevibus laxiusculis, lacinia calycis dorsali corolla longiore." The genus *Ætheilema*, separated from *Ruellia* by Mr. Brown in his Prodromus, he has since found to be the same with Willdenow's *Phaylopsis*. This plant was raised by the Horticultural Society from seed from Sierra Leone. *Prostanthera lasianthos*, a New Holland plant: Mr. Brown has recorded 13 species of this genus. *Iris neglecta. Salvia nutans. Polygala amara.* This species and *vulgaris* are often mistaken for each other, from their variableness, and the difficulty of finding good distinguishing characters: the taste, however, will at once decide, the *vulgaris* being slightly acrid without bitterness, whilst the *amara* is intensely bitter. *Polygala cordifolia*, the *fruticosa* of Bergius:—Cape of Good Hope. *Protæa lævis*, from the mountains at the Cape of Good Hope. This is the same plant which Mr. Salisbury gave in the *Paradisus Londinensis* as *longifolia*, "a name already occupied by a very different species, of which there are three varieties figured in the Botanist's Repository." *Rauwolfia ternifolia.* Collected in South America by Humboldt and Bonpland, and described but not figured by them.

The Botanical Register. No. 104.

Descriptions of Plates 725—739 given in preceding numbers: *Schizanthus pinnatus*, now first drawn from the living plant; the figure of Ruiz and Pavon, who established the genus *Fl. Peruv.*, being from a dried specimen. *Astelma fruticans*, the *Gnaphalium fruticans* of *Hort. Kewensis*. *Oncidium luridum*, "foliis ellipticis acutis, scapo stricto ramoso, perianthii laciniis patentibus undulatis retusis subæqualibus, labello reniformi, columnæ alis rotundatis." An unrecorded species from South America. *Daviesia alata*, Smith Linn. Trans. ix. A very rare plant. *Berberis Chitria*, the *aristata* of Decandolle, *Syst. Veg.* Under this head we find some animadversions on the work of the Genevan professor in a spirit of asperity and of exultation at the presumed failure of his undertaking (too great, no doubt, for any man to hope to accomplish), which we wish had been spared, and which may perhaps call for some observations at a future time. *Brexia madagascariensis*, a species not described in any general system of vegetables. *Alstrœmeria Flos Martini*, *pulchra* of Bot. Mag. See p. 224 of our last Number. "The drawing was taken at the garden of the Horticultural Society, enriched, extended, and arranged under the able direction

rection of the intelligent and indefatigable secretary, Mr. Sabine; next to whom, we must not forget, in their different departments, Messrs. Lindley and Monroe. In our opinion, that richly-endowed establishment cannot be confided to abler or more competent agents, as well in regard to the application of its treasures, as a judicious management of the collection." We are glad to transcribe this testimony as conveying our own sentiments. *Dendrobium squalens*, "terrestre bulbis conicis truncatis, floribus resupinatis confertis, foliis lanceolatis plicatis subtrinervibus scapo duplo longioribus. *Lindley MSS.*" Sent to England from Rio de Janeiro by Mr. Forbes, a collector in the service of the Horticultural Society. *Lobelia campanuloides*, newly introduced from China. Thunberg in Linn. Trans. ii. 332. *Dianella longifolia*, Brown's Prodr. i. 280, now first figured. *Gardenia amœna*, lately figured in Bot. Mag. *Erythrina caffra*, Thunb. Prodr. *Passiflora herbertiana*, with an appropriate specific character of six lines, too long for us to transcribe. *Edwardsia chrysophylla*, Linn. Trans. ix. 299. *Rosa involucrata* of Mr. Lindley's Monograph.

Pl. 740. *Nemophila phacelioides. Bignonia æquinoctialis β.* given as a separate species, *Chamberlaynii* in Bot. Mag. *Eulophia gracilis*, "scapo gracillimo, foliis lanceolatis trinerviis triplo longiore, calcare clavato, labelli lobo medio obsoleto. *Lindley's MSS.*" In the garden of the Horticultural Society, sent from Sierra Leone last year by Mr. G. Don. *Phaseolus semierectus. Calceolaria integrifolia. Isochilus linearis. Iatropha gossypifolia. Tritonia flava*; recorded by Dr. Solander in *Hortus Kewensis* under *Gladiolus*, "from which genus it was detached by us*, in the treatise on Ensatæ in the Annals of Botany, i. 219."

LXIV. *Proceedings of Learned Societies.*

HORTICULTURAL SOCIETY OF LONDON.

Oct. 7.—THE following communications were made:

On the Form and Materials for Rafters and Bars for the Roofs of Hot-houses, &c.; by Mr. Thomas Tredgold, Civil Engineer.

The rafter proposed by Mr. Tredgold is of iron with a casing of wood, the advantage of which is, that its dimensions may be much smaller than if made wholly of wood; and the objection to iron rafters or bars is effectually remedied, namely, the facility which they give to the escape of caloric. Mr.

* Ensatarum Ordo, autore Joh. Bellenden Gawler, armigero.

Tredgold

Tredgold also proposes that the strength of the rafter shall be obtained by making it flat rather than deep, as he conceives, that the great depth of the rafter produces a shade in the house at the period when the sun is low in the horizon; and at the time when he is at his greatest altitude the obstruction of his beams by the flat rafter will rather be advantageous than otherwise.

Description of a Vinery constructed upon a new Plan, by William Atkinson, Esq., and an Account of the Mode of Training practised in it. By Mr. William Beattie, Corresponding Member of the Society. The excellence and economy of Mr. Atkinson's plan of constructing Vineries, is now very generally ascertained. By having introduced an easy and complete mode of ventilation, he has rendered it unnecessary to make the sashes moveable, and thus avoids the continual liability to breakage, which there is with moveable lights.

METEOROLOGICAL SOCIETY OF LONDON.

The science of Meteorology is peculiarly susceptible of improvement, by means of a combined system of experiment and observation, carried on under the auspices of an associated body of inquirers. It embraces an immense variety of atmospheric phænomena, presented to our view under multiplied relations, modified in innumerable ways by the various configurations of the earth's surface, and connected, perpetually and intimately, with the subjects of almost every branch of scientific investigation. This character of the science, and that more particularly when considered with reference to its present defective state, clearly evinces the propriety, and even the necessity, of giving to the pursuit of Meteorology a new and determinate form, by affording it that powerful aid,—the establishment of a Society expressly devoted to its cultivation, —which experience shows to have been so effectual in promoting the advancement of every department of knowledge to which it has been applied.

It is under this impression that we have much satisfaction in announcing the formation of the "Meteorological Society of London," which took place on Wednesday the 15th instant, at a meeting held for the purpose, pursuant to the notice which was inserted in our last Number. The following account of the preliminary arrangements agreed to on the occasion, has been transmitted to us by the Provisional Committee; and we have now only to express our cordial wishes for the prosperity of the undertaking, and our hopes, that this Society, closely and harmoniously allied by its extensive objects of

of research, with every other philosophic association, but infringing on the province of no one, may be eminently successful, in its own department, in extending the boundaries of human knowledge.

On the 15th inst. a Meeting was held at the London Coffee House, Ludgate Hill, to take into consideration the propriety of forming a Meteorological Society. Among the gentlemen present were Drs. T. Forster, Clutterbuck, Shearman, Mr. Luke Howard, &c. &c. At eight o'clock the Chair was taken by Dr. Birkbeck, when the following Resolutions were agreed to:—

1. *Resolved*, That the formation of a Society to promote the advancement of Meteorology, have the cordial approbation of this Meeting.

2. *Resolved*, That a Society be formed to be called "The Meteorological Society of London."

3. *Resolved*, That the business of this Society shall be conducted by a President, Vice-Presidents, Treasurer, Secretary, and Council; and that the number of Vice-Presidents and Members of the Council be determined at a subsequent Meeting.

4. *Resolved*, That Mr. Thomas Wilford be requested to officiate as Secretary to this Society (*pro tempore*), and that he be authorized to send a printed Summons to attend the next Meeting to each person who shall become a Subscriber.

5. *Resolved*, That an Annual Subscription of Two Guineas be paid in advance by every Member of this Society.

6. *Resolved*, That those gentlemen present who are inclined to become Members of this Society, do now send their names to the Secretary to be enrolled.

7. *Resolved*, That a Committee of three Members be appointed, in conjunction with the Secretary, to draw up an account of the Society's proceedings this evening.

8. *Resolved*, That scientific men throughout the United Kingdom be solicited to co-operate with this Society, and to transmit communications to it; and that this Society will always be ready to receive meteorological observations from the cultivators of science throughout the various quarters of the globe.

9. *Resolved*, That no other qualification be required to constitute eligibility to this Society, than a desire to promote the science of Meteorology.

10. *Resolved*, That after the next Meeting the election be by ballot upon the proposition of three, and that a majority of Members decide.

11. *Resolved*, That this Meeting do adjourn to the 12th of November next, to meet at the same place and hour.

MEDICO-

MEDICO-BOTANICAL SOCIETY.

The Medico-Botanical Society of London held its first Meeting this Session on Friday, Oct. 10. An address was delivered to the members on the objects and utility of the Institution; after which the death of its late honorary member, Dr. Baillie, was notified to the Society, accompanied by an appropriate eulogium on his character. The Meeting then adjourned to Oct. 31, 1823.

ROYAL ACADEMY OF SCIENCES OF PARIS.

June 30.—M. Gaillon of Dieppe communicated some microscopic and physiological Experiments on a Marine Conferva; and M. Arnoul a Memoir on Equations of three terms.

MM. Cauchy and Ampere gave an account of a Memoir, by M. Texier de Montainville, on the Inscription of the Cube in the Octahedron. The author shows that this problem is undetermined. If we take for the axes of x, y, z, the three diagonals of an octahedron, every section made in this octahedron by a plane parallel to x, y, will be a square; and if double the distance of the two planes is contained between the side of the square and its diagonal, it is clear that in the square treated of may be inscribed a second, which will become the base of a cube inscribed in an octahedron; and what is remarkable is, that the summits of all the cubes inscribed in this manner trace on each face of the octahedron an equilateral hyperbola.

M. Arago gave an account of the experiments which Mr. Wheatstone had just been making in England on Phonic Vibrations.

M. Giraud read a third Memoir on Navigable Canals, considered with regard to the fall and distribution of the locks.

M. Geoffroy-Saint-Hilaire read a Note on the Respiration of the Fœtus.

M. Longchamp read a Memoir on the Analysis of Phosphoric Acid, and of Phosphates.

LXV. *Intelligence and Miscellaneous Articles.*

HISTORY OF THE REDISCOVERY OF ENCKE'S COMET.

THE merit of the rediscovery of this comet *, which has excited great interest, is due to our countryman, Mr. James Dunlop,

* See Phil. Mag. lxi. p. 275—282.

Dunlop, an ingenious maker of telescopes from Ayrshire, who went out to New South Wales with His Excellency Sir Thomas Brisbane, as a scientific assistant. Mr. Dunlop was examining the heavens with a sweeper, when he encountered this singular body. We state this fact on the authority of Sir Thomas Brisbane, who has recently transmitted to the Royal Society of Edinburgh a series of valuable astronomical observations made at Paramatta. It is impossible to speak too highly of the zeal and talents of this eminent astronomer, whose appointment to the government of New South Wales has given such universal satisfaction. Great credit is due to him in doing this justice to our modest countryman. Baron de Zach, who considers the rediscovery of this comet as one of the greatest efforts of modern astronomy, ascribes all the glory of it to the "vigilant and penetrating eye of M. Rumker," and to "Germanic diligence." M. Rumker has great merit in every thing he does, and particularly in what he has done on this subject; but the merit of discovering the comet is solely Mr. Dunlop's.—*Edin. Phil. Journ. vol.* ix. *p.* 391.

NEW ELEMENTS OF ENCKE'S COMET.

The following correct elements of this comet have been given by M. Encke:

Passage of the perihelion, 1822, May 21, ·01768, mean time at Seeberg.

Longitude of the perihelion.........	157° 11′ 28″·8	From mean equinox.
——————— node...............	331 19 31 ·9	
Inclination of the orbit,		
Excentricity...........................	0·8445479	
Its sine............................	57° 37′ 24″·7	
Log. of one-half the greater axis...	0·3472191	

M. Encke is engaged in very laborious calculations, with the view of ascertaining if the resistance of the ether could have any influence in causing the diminution which has been observed in its periodical time.—*Edin. Phil. Journ. vol.* xi. *p.* 391, *from Zach's Corresp. Astron. vol.* viii. *p.* 279.

ANSWER TO MR. J. HAMETT'S QUESTION
[in our last Number, p. 236].

Newark, Oct. 9, 1823.

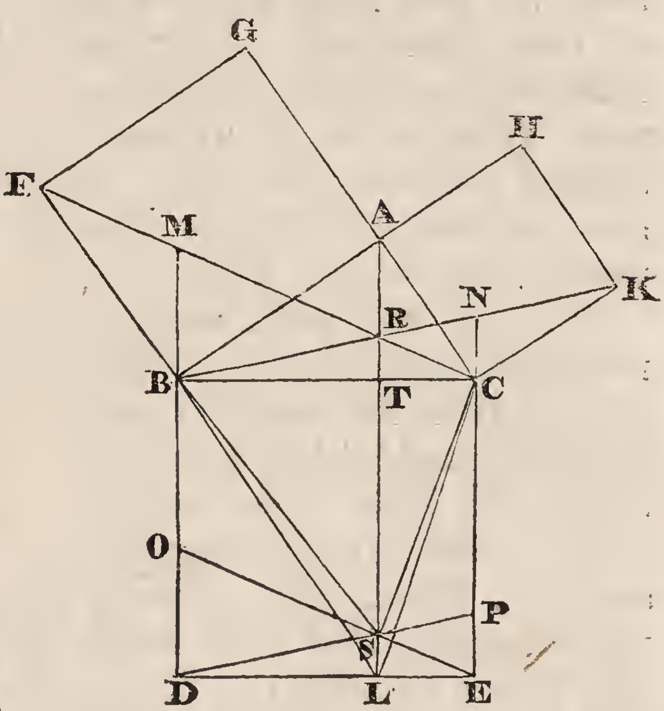
Although Mr. Hamett's question appears to rank among those of the axiomatical class, yet I have endeavoured to comply with his wish by sending the following demonstration, which

which is much at your service, if it should be thought to merit insertion in your valuable Journal.

I am your obedient servant,

PAUL NEWTON.

In prop. 47, produce DB to meet FC in M, and EC to meet KB in N. From the point E draw EO parallel to CM; and from D draw DP parallel to BN (prop. 31st): Then will the parallelograms DBCE, DBNP, and ECOM, be equal to one another (props. 35 and 36). The parallelograms DBTL, DBRS, and OMRS, are also equal to one another. For the same reason, the parallelograms LTCE, SRCE, and SRNP, are equal to one another. Now LT is equal to SR (prop. 34); consequently LS is equal to TR. But the side SR is evidently common to the four parallelograms DBRS, OMRS, ECRS, and PNRS; therefore the lines FC, KB, and AL, intersect one another in the point R; or otherwise the opposite sides of parallelograms could not be equal. But further; draw BL, BS, CL, and CS. The triangle FBC is equal to the triangle BDL (props. 34 and 47), or equal to the triangle BSR (props. 37 and 38). For the same reason, the triangle BKC is equal to the triangle CLE, or is equal to the triagle CSR. The three triangles LDS, LBS, and TBR, are equal to one another (props. 37 and 38). Again, the three triangles LES, LCS, and TCR, are equal to one another. Hence we perceive that the two triangles LBS and LCS, which meet in the point S, on the line LA, are respectively equal to the two triangles DLS and ELS, which meet likewise in the point S; or they are respectively equal to the two triangles TBR and TCR, whose sides BR and CR meet in the point R, on LA. Besides, we perceive that because OM is equal to BD, BM is equal to OD. Therefore the triangles MRB and OSD are equal to each other, and the triangles NRC and PSE are for a similar reason equal to each other (prop. 38); consequently the lines FC, KB, and AL, intersect one another in the point R.



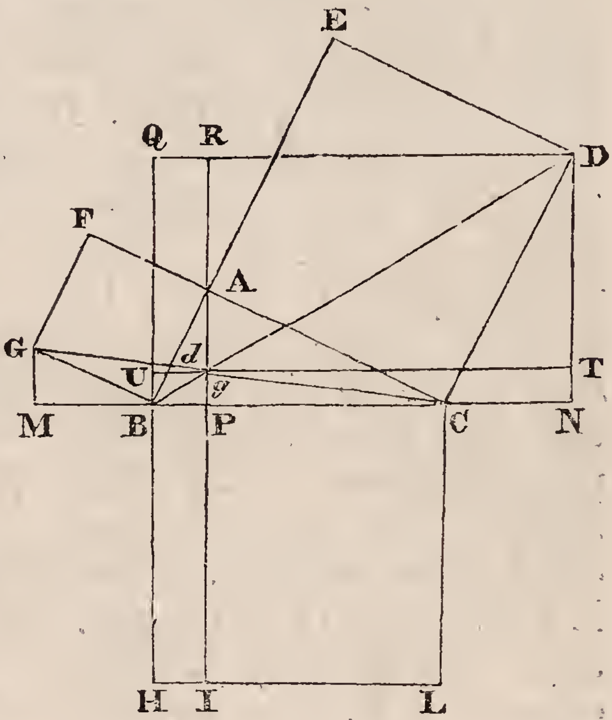
Q. E. D.

SOLUTION

SOLUTION OF MR. HAMETT'S QUESTION. BY MR. M. N. CRAWFORD.

Cranford, Oct. 9, 1823.

Let ABC be the given triangle right-angled at A. Upon AB, AC, BC, describe the squares AG, AD, and CH. Then a right line being drawn from A parallel to BH will meet the two lines GC, BD in their common point of intersection, which Mr. Hamett requires to be demonstrated without the aid of any proposition of the Elements beyond the 47th.



Produce BC both ways to meet perpendiculars on it from G and D in M and N; and since the angle ABG is a right angle, the sum of the angles GBM, ABP is equal to a right angle (1 Elem. 13). Hence the angles at M and P being right angles, and GB equal to BA, the triangles GBM, BAP are identical (1 Elem. 26), and BM is equal to AP, and GM to BP. In a similar manner it may be proved that the triangles DNC, CPA are identical, and that DN is equal to CP, CN to AP and consequently to MB; and hence CM to BN.

Complete the parallelogram BNDQ, and through *d* the intersection of AT and BD draw TU parallel to BC, and produce PA to R. Then because the complements Q*d*, *d*N are equal (1 Elem. 43), the parallelograms QP, UN are equal; that is, a parallelogram whose base and altitude are P*d*, PN is equal to a parallelogram whose base and altitude are BQ, BP or PC, BP. In the same manner it may be proved that the parallelogram whose base and altitude are CM, P*g* (the segment of AP cut off by CG) is equal to the parallelogram whose base and altitude are PC, BP. Consequently the parallelogram whose base and altitude are P*d*, BN is equal to the parallelogram whose base and altitude are P*g*, CM. Therefore, as CM, BN, have been shown to be equal, P*g* is equal to P*d*; that is, the intersection of CG, AP, coincides with the intersection of BD, AP.—Q. E. D.

MERVYN NOTT CRAWFORD.

LONGITUDE AND LATITUDE OF PARAMATTA.

The longitude of the observatory of Paramatta, in New South Wales, is $10^h\ 4'\ 14''\cdot5$ east of Greenwich, as determined by various methods of observation. The latitude of the observatory is $33°\ 48'\ 42''$.—*Edin. Phil. Journ. vol.* ix. *p.* 391.

OBSERVATORY OF DORPAT IN LIVONIA.

This observatory, under the direction of M. Struve, an able and active astronomer, has been supplied, in the most handsome manner, with fine instruments, by the Emperor of Russia, whose liberality to science deserves the highest encomiums. M. Frauenhofer of Munich has been occupied for two years in completing, for this observatory, an achromatic telescope, *fourteen feet in focal length, and with an aperture of nine inches.* "You may judge from this," says M. Struve in a letter to Baron de Zach, "how much our liberal Government does for astronomy. Our observatory is particularly indebted to the curator of our university, M. General Comte de Lieven, who has not only provided it with every thing that is excellent and perfect in the way of instruments, but has also built a commodious house for the astronomer. He has likewise ordered a great meridian circle, similar to that of Gottingen, Munich and Konigsberg; a great repeating circle; and an universal instrument, &c., all from the manufactory of MM. Reichenbach and Ertel of Munich.—*Edin. Phil. Journ. vol.* ix. *p.* 392, *from Zach's Corres. Astron. vol.* viii. *p.* 370.

MEASUREMENT OF A DEGREE IN LIVONIA.

The liberality of the Russian Government has also been shown, in charging M. Struve of Dorpat, with the measurement of a degree of the meridian in Livonia. Properly speaking, this work is carried on by the University out of the large funds which the Government has put at its disposal for every purpose that is useful and interesting to science. M. Struve began his operations in the summer of 1822.—*Edin. Phil. Journ. vol.* ix. *p.* 392.

THE GREENWICH MURAL CIRCLE.

Feeling a lively interest in any thing connected with the Royal Observatory, we have, with the greatest satisfaction, seen the results of Mr. Pond's inquiry into the state of the Greenwich mural circle: the experiments prove almost to a mathematical certainty, that this splendid instrument is, after twelve years' constant use, as free from error, as even its warmest advocates, or the most accomplished observer, could wish.—*Journal of Science, vol.* xvi. p. 189.

MR. GROOMBRIDGE'S TRANSIT CIRCLE.

Whilst admiring the mechanical skill of him who constructed the Greenwich mural circle, we were much concerned to hear that there were some grounds to suspect the accuracy of another instrument made by the same artist, and generally considered little inferior to the Greenwich circle itself; we allude to the four-feet meridian transit circle, late the property of Mr. Groombridge. On this gentleman's retiring from the duties of an active observer, the instrument was disposed of, liable, however, to an examination on the part of its maker, as to its efficiency or inefficiency; which investigation being conducted by Mr. Troughton, in the presence of Mr. Groombridge, the late Professor Tralles, and its intended purchaser, gave reason to fear that some alteration in its figure had been sustained. Accordingly, future and more minute examination was deemed necessary; and at length it was resolved, that comparisons of north polar distances taken on the same nights with it and the Greenwich mural circle should be entered into; and the results of many weeks' observations proved, that those obtained by Mr. Groombridge with *his* instrument, were, to use the words of the Astronomer Royal, " as coincident with those procured by the Greenwich mural circle, as those of the Greenwich mural circle were with themselves." Knowing that the reports of the suspected inaccuracy have extended far and wide, we feel it due to Mr. Troughton who constructed the instrument, and to Mr. Groombridge who used it, to give publicity to the above statement. It is at present in Blackman-street, and is having eight additional microscopes applied by Mr. Troughton; it will then have six readings to each of its divided circles, so that all error of division will probably be annihilated. We hope ere long to see it actively employed.

RETURN OF THE EXPEDITION UNDER CAPTAIN PARRY.

At length the increasing anxiety for the fate of our brave countrymen who have been so long exploring the Polar Seas, has been terminated by their safe return. The Fury and Hecla arrived at Lerwick, in Shetland, on the 10th instant, made the northern coast of England on the 16th, and on Saturday, the 18th, the gallant and enterprising Captain Parry reached London. He and his brave companions have well earned the admiration of their countrymen and of all mankind, although the discovery of the long-sought northwest passage has not yet been the reward of their exertions.

The outward voyage in 1821 was fair and prosperous. Passing up Hudson's Straits, the navigators kept near the land on their south, and explored the coast towards Re-

pulse

pulse Bay. The furthest west which they attained was 86° of longitude, and the highest latitude only 69° 48′ N; and they finally brought up for winter-quarters at a small isle which they named Winter Island, in 82° 53′ W. longitude, and latitude 66° 11′ N.

The chief part of the summer of 1821 was occupied in examining Repulse Bay, and some inlets to the eastward of it, through some one or other of which they hoped to find a passage into the Polar Sea. In this they were disappointed, for all the openings proved to be only deep inlets, which ran into the continent of America. While thus occupied, early in October the sea began to freeze: and on the 8th of that month the ships were laid up for the winter in the situation noted above. Here at Winter Island the Expedition was frozen up from the 8th of October 1821 to the 2d of July 1822.

The most beneficial effects resulted from the system of heating the ships with currents of warm air. These were directed to every requisite part by means of metallic tubes, and so well did the contrivance answer its purpose, that the lowest temperature experienced during the winter was 35° below zero. In the second winter it was ten degrees lower, viz. 45° below zero; but this was not near so difficult to endure, nor so inconvenient, as the cold in Capt. Parry's first voyage, nor indeed, if we are rightly instructed, as that felt in the northern stations of the Hudson's Bay traders on the American continent.

In the season of 1822, the vessels having steered along the coast to the north, penetrated only to the long. of 82° 50′, and lat. 69° 40′; and, after exploring several inlets &c. in their brief cruize, they were finally moored for their second winter, about a mile apart, in 81° 44′ W. long. and lat. 69° 21′ N. Here, close to another small isle, they remained from the 24th of September 1822 to the 8th of last August. They had latterly entered a strait leading to the westward. From the accounts of a party of Esquimaux and their own observations, they had every reason to believe that this strait separated all the land to the northward from the continent of America. After getting about fifteen miles within the entrance of it, however, they were stopped by the ice; but from the persuasion that they were in the right channel for getting to the westward, they remained there for nearly a month, in daily expectation that the ice would break up. In this last hope they were again quite disappointed, and on the 19th of September, the sea having begun to freeze, they left these straits, and laid the ships up in winter quarters near the small island alluded to, and called by the Esquimaux Igloolik.

The inlet where the second winter was spent presented a

 solid

solid mass of perpetual ice. It is about ten miles in breadth; its length (of course not having been traversed) uncertain. The ebb tide is from the south-west, and the flood from south-east; small channels ran through it, but not wide enough to work a ship.

ON FELSPAR, ALBITE, LABRADORE SPAR, AND ANORTHITE. BY GUSTAVUS ROSE, OF BERLIN.

Some differences which Mr. Rose observed in the angles of certain crystals, hitherto classed among the felspars, led him to make a closer investigation of them; the result of which was, that under these crystals are contained four species, differing both in a crystallographical and chemical point of view, though in the former respect they exhibit an undoubted analogy.

Felspar proper, $KS^3+3\,AS^3$, is the most abundant of these species. To it belong the Adularia of St. Gothard, the glassy felspar of Vesuvius and the Siebengebirge, the Amazon-stone of Siberia, the Labradore felspar from Friedrichswärn in Norway, the felspar of Baveno, Carlsbad, and the Fichtelgebirge, and generally most part of Werner's common felspars.

The second species, Albite, is more rare. It is denoted by $NS^3+3\,AS^3$. Eggerts first found it in an uncrystallized fibrous and granular form at Finnbo and Broddbo, near Fahlun, and thereafter Haussmann and Stromeyer in a mineral from Chesterfield, in North America, to which the former gave the name of Kiefelspath. Nordenskiold found it in a granite at Kimite, near Pargas, in Finland; and Ficinus in a granite from Penig in Saxony. All these are uncrystallized varieties. To the crystallized, which I have had occasion to see, belong the white schorl, first described by Romé de l'Isle; the felspar crystals of Dauphiny of Haüy; the small crystals from Saltsburg and the Tyrol, known a few years ago under the name of Adularia.

The third species forms the Labradore spar, which Klaproth analysed and distinguished from felspar, though mineralogists did not consider it as a distinct species. Berzelius has assigned to it the formula $NS^3+3\,CS^3+12\,AS$ from Klaproth's analysis.

The fourth species is the rarest of the whole. Mr. Rose has recognised it only in the druses of limestone blocks, which are found at Mount Somma, near Vesuvius, where it occurs in small shining perfect crystals. He has determined its formula to be $MS+2\,CS+8\,AS$; and has called it Anorthite.

Albite is readily distinguishable by the twin grouping of its crystals. Its primitive form is an irregular parallelopiped. In

In its massive state it differs from felspar in not being straight foliated, but always radiated. Labradore spar is completely decomposed by concentrated muriatic acid, while felspar and albite are not affected by it. Anorthite yields to muriatic acid as Labradore spar does. The name is derived from ἄνορθος, not rectangled; as the want of a right-angled cleavage, in both directions of its laminæ, peculiarly distinguishes it from felspar. We must refer to the paper itself for the details of the crystallization-system of the above minerals. — *Journal of Science*, vol. xvi. p. 106, from *Gilbert's Annalen*, No. lxxiii. p. 173.

METEOR AND EARTHQUAKES.

At Ragusa (in Dalmatia) the heat in August last was at 31° of Reaumur, which produced contagious diseases, that carried off a great number of people. The drought was very distressing. On the 20th of that month the air became suddenly dark, a fiery meteor appeared over the city, fell into the sea, and was followed by an earthquake, which overthrew many houses. Several persons were killed. The sea retired nearly a mile from the coast. The first shock was felt in Turkish Bosnia: it caused an immense piece of rock to fall, which, rolling into the sea, struck a vessel laden with flour and buried it with its crew in the waves. It is reported that a volcano has broken out in that province. At Ragusa a fort built by the French, and a great number of houses, are thrown down.

Accounts from St. Petersburgh state, that slight shocks of an earthquake were felt at Pawlouisk, in the government of Wororesch, on the 22d, 23d, and 27th of August.

STORM AT ROTTERDAM.

Dublin, October 20, 1823.

In your Magazine for last month you gave an account of the effects of a storm in the districts of country round Antwerp in August last, where your correspondent says some hundred trees were overturned and great ravages committed in the cornfields and gardens by water-spouts; and one place is mentioned where twenty large trees were broken by these spouts and thrown across the public road. I happened to be in that part of the country at the time, and I did not hear of any damage done by water-spouts, nor did I see any marks of their ravages on the fields; but there were some severe thunder storms at that time. And near Mechlin, on the road side, I counted thirteen large trees broken across and lying by the way. They had been broken *by lightning* a few days before I passed, and were part of a row of poplars which had lined the road. And, what appeared to me very singular, it was

only each alternate tree that had been struck, one being broken and one left. The road had, as you say, been blocked up with them, so that the diligences for the day could not get on.

One of these thunder storms occurred at Rotterdam a few days previous, and presented in its progress some interesting and beautiful phænomena. The day (26th Aug.) had been excessively hot and sultry, with the wind at SSE.; at 4 p. m. clouds began to approach from the NW. and some thunder was heard in that quarter. At 5 it came nearer, and the lightning from the north was frequent. Clouds then suddenly began to drive from the east, carrying with them along the ground a vapour like blue smoke, which rose upwards and soon became tinged of a deep dusky red. The lightning was now continual, the air seemed on fire, and the thunder rolled in one unbroken and unceasing peal. It grew very dark, and the rain poured down in torrents. The storm passed directly over head, but at a great height, and the lightning did not strike the earth. The air shortly after became clear to the NW., the thunder cloud slowly retiring in a SE. direction, when it seemed to become fixed at the distance of six or eight miles, and at the height of about 25°, and there the storm was seen exerting its fury in the highest splendour. The cloud was one blaze of fire, and the flashes of lightning darted from one quarter of it to another in the most fantastic coruscations; sometimes zigzag, at others in streams of fire or running out in circular lines of blue flame, or darting from it like the forked lightning which painters put into the hands of Jupiter. This fine display of fireworks continued more than an hour; the moon in the mean time rose behind the cloud in great majesty, and began to move along the sky, which was calm and serene in every other quarter. There was no thunder heard, and the streets and walks of Rotterdam were filled with admirers of this interesting spectacle. Yours, &c.

W. W. Jameson.

BRITISH TENTHREDOS.

A young Entomologist who makes inquiry in the Philosophical Magazine for August, p. 155, concerning the British *Tenthredos*, and requests their specific names and characters, is probably not aware of the number of British species.—An entomologist of the first eminence informs us that his cabinet contains about 150; and adds that there may probably be double that number, were all known. We believe that the collections of Mr. Haworth and Mr. Stephens are equally extensive. We know not, however, whether the inquiry relates to the genus

Tenthredo

Tenthredo of modern entomologists, or to the Linnæan genus, which constitutes their family *Tenthredinidæ*, including 23 British genera, according to Dr. Leach's division, as given in Mr. Samouelle's Entomologist's Compendium.

CUTTING OF STEEL BY SOFT IRON.

Mr. Barnes, of Cornwall, Connecticut, has ascertained a singular property of soft iron in cutting hard steel. He had fixed a circular plate of soft sheet iron on an axis, and putting it into a lathe, gave it very rapid rotatory motion, applying, at the same time, a file to it to make it perfectly round and smooth; the file, however, was cut in two by the plate, the latter remaining untouched; and it was found not to have been much warmed in the operation, though a band of intense fire surrounded it whilst in action.

A saw made of a very hard plate, which required altering, was cut through longitudinally in a few minutes, and afterwards teeth were cut in it by the same means. Had the file been used to produce the same effect, it would have required a long and tedious operation.

Rock crystal applied to the plate cut it readily.—*Silliman's Jour.* vi. 336.

Mr. Jacob Perkins, of Fleet-street, has verified this remarkable and useful observation. A piece of a large hard file was cut by him into deep notches at the end, where, also, from the heat produced by friction, it had softened and been thrown out like a bur. On another part of the file, where the plate had been applied against its flat face, the teeth were removed, without any sensible elevation of the temperature of the metal. The plate, which had previously been made true, was not reduced either in size or weight during the experiment, but it had, according to Mr. Perkins, acquired an exceeding hard surface at the cutting part.—*Journal of Science*, xvi. 155.

PURPLE TINT OF PLATE GLASS AFFECTED BY LIGHT.

"It is well known," Mr. Faraday remarks, "that certain pieces of plate glass acquire, by degrees, a purple tinge, and ultimately become of a comparatively deep colour. The change is known to be gradual, but yet so rapid as easily to be observed in the course of two or three years. Much of the plate glass which was put a few years back into some of the houses in Bridge-street, Blackfriars, though at first colourless, has now acquired a violet or purple colour. Wishing to ascertain whether the sun's rays had any influence in producing this change, the following experiment was made: Three pieces of glass were selected, which were judged capable

pable of exhibiting this change; one of them was of a slight violet tint, the other two purple or pinkish, but the tint scarcely perceptible except by looking at the edges. They were each brokeni nto two pieces; three of the pieces were then wrapped up in paper and set aside in a dark place, and the corresponding pieces were exposed to air and sunshine. This was done in January last, and the middle of this month (September) they were examined. The pieces that were put away from light seemed to have undergone no change; those that were exposed to the sunbeams had increased in colour considerably; the two paler ones the most, and that to such a degree, that it would hardly have been supposed they had once formed part of the same pieces of glass as those which had been set aside. Thus it appears that the sun's rays can exert chemical powers even on such a compact body and permanent compound as glass."

CHANGE OF FAT IN PERKINS'S ENGINE BY WATER, HEAT, AND PRESSURE.

Mr. Perkins uses in his steam cylinder a mixture of about equal parts of Russia tallow and olive oil to lubricate the piston and diminish friction. This mixture is consequently exposed to the action of steam at considerable pressure and temperature, and being carried on by the steam, it is found in the water, giving rise to peculiar appearances. The following is Mr. Faraday's account of it.

The original mixture is solid at common temperatures, but fuses at about 85° Fah. When boiled in alcohol, a small portion dissolves.

The water, as it issues from the end of the ejection-pipe into the tub placed to receive it, and from which it is pumped up again into the generator, appears white and translucent, and after having been used some time, very much resembles thin milk. A scum is found floating on it, which, when collected together, forms a soft solid, but when it has been long exposed to the action of the steam and at a high temperature, is hard like wax nearly. It is always black and dirty. A portion of this substance was digested in hot alcohol, and the clear solution set aside; flocculi separated in abundance from it on cooling, which, when dried, collected, and fused, gave a grayish substance, contracting and cracking as it cooled, with the lustre and appearance of wax, but rather more brittle. It does not melt in boiling water, but at a higher heat melts, and ultimately burns like fat. It is rather lighter than water; it dissolves readily in alkalies, more readily, I think, than fat, and in this respect resembles Chevreul's acids of fat, as well

as

as in its solubility in alcohol; the alkaline solution is turbid. It is not soluble in ether, or very slightly so; when burnt it leaves an ash consisting principally of carbonate of lime.

The cold alcoholic solution, on evaporation, left a substance similar in many respects, but much softer, even fluid. It burnt in the same manner, leaving a slight ash of carbonate of lime. The merest trace of copper was found in these substances.

The action of the alcohol being continued, nothing at last remained but dirt and mechanical impurities. The softer portions from the surface of the water were found to contain a quantity of unchanged fat and oil.

The milky water, on examination, was found to be a mixture, probably, of this substance and water. It undergoes no change in appearance when left for many weeks; but when filtered through good filtering paper, the latter portions came through clear and transparent, the altered fat being separated. When evaporated, it leaves a substance having all the properties of the solid matter above described. The finely-divided state of the substance, its solidity, and its near approach to the specific gravity of water, will, perhaps, account for the length of time during which it will remain uniformly diffused through it.—*Journal of Science*, xvi. 172.

LIST OF NEW PATENTS.

To John Christie, of Mark-Lane, London, merchant, and Thomas Harper, of Tamworth, Staffordshire, merchant, for their improved method of combining and using fuel in stoves, furnaces, boilers and steam-engines.—Dated 9th of October 1823.—2 months allowed to enrol specifications.

To Joseph Rogerson Cottor, of Castle Magnor, near Mallow, in the county of Cork, for certain improvements on wind musical instruments.—9th October.—6 months.

To John Henfrey, of Little Henry-street, Waterloo Road, Surry, engineer, and Augustus Applegath, of Duke-street, Stamford-street, Blackfriars, Surry, printer, for certain machinery for casting types.—9th October.—4 months.

To Edward Schmidt Swaine, of Bucklersbury, London, (in consequence of a communication made to him by Frederick Adolphus Augustus Streeve, of Dresden, doctor of physic, and Edward Swaine, of Leipsig, merchant, on whose behalf he is pursuing the patent,) who is in possession of an invention for a method of producing and preserving artificial mineral waters, and for machinery to effect the same.—9th October.—6 months.

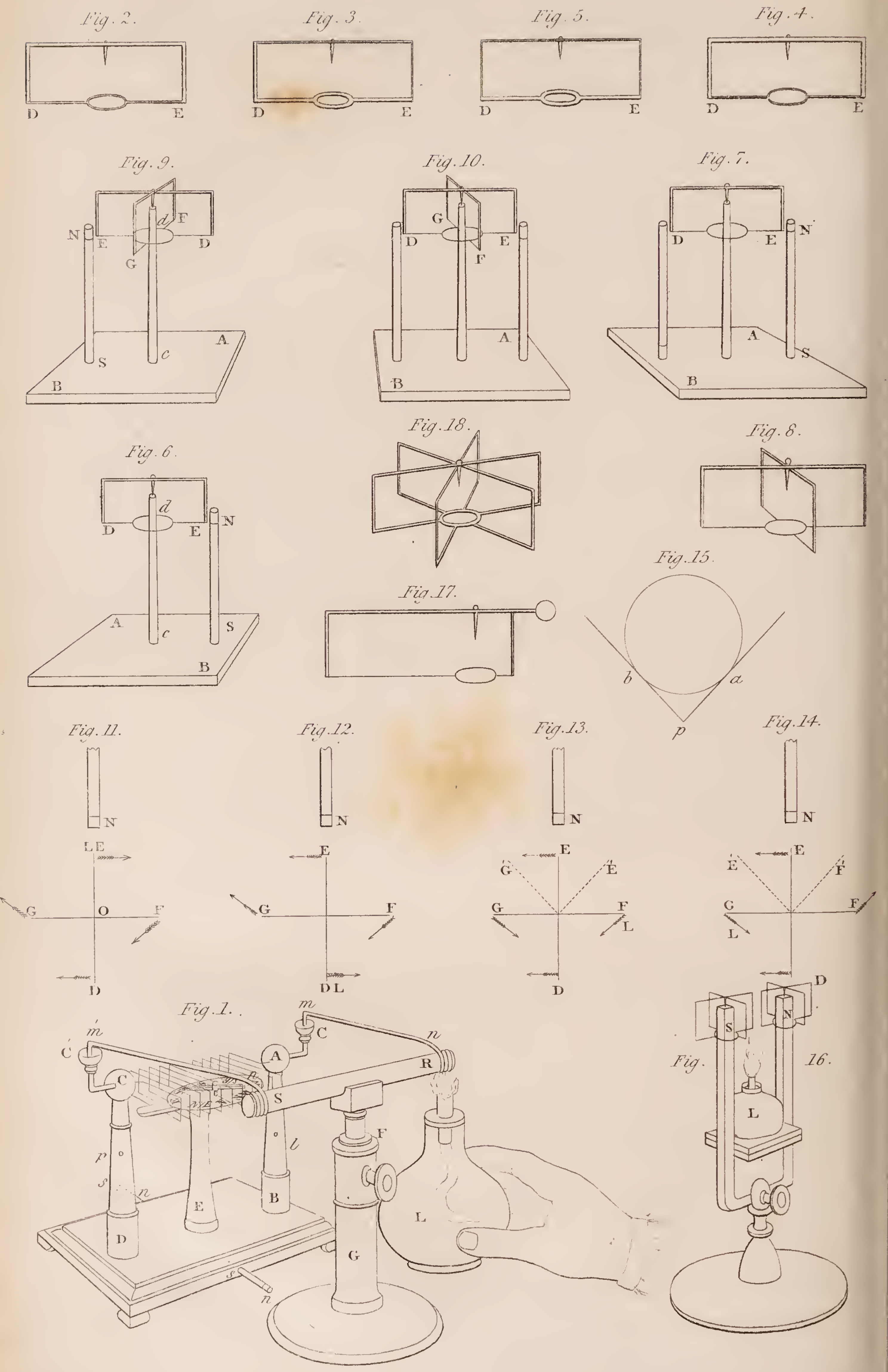
To Sir William Congreve, of Cecil-street, Strand, Middlesex, baronet, for his various improvements in fire-works.—16th October.—6 months.

To Archibald Buchanan, of Cathrine Cotton Works, one of the partners of the house of James Finlay and Company, merchants, in Glasgow, for his improvement in the construction of weaving looms impelled by machinery, whereby a greater quantity of cotton may be woven in a given time without injury to the fabric than by any application of power for that purpose heretofore employed.—16th October.—2 months.

METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr.* BURNEY *at Gosport; Mr.* CARY *in London, and Mr.* VEALL *at Boston.*

Days of Month, 1823.		Gosport, at half-past Eight o'Clock, A.M. Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Clouds. Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Height of Barometer, in Inches, &c. Lond. 1 P.M.	Bost. $8\frac{1}{2}$ A.M.	Thermometer. London. 8 A.M.	Noon.	11 P.M.	Boston. $8\frac{1}{2}$ A.M.	Rain. London.	Boston.	Weather. London.	Boston.
Sept.	26	29·90	62	$53\frac{1}{2}$	68	SW.	...	......	...	1	1	...	1	1	...	29·90	29·50	60	66	52	58	...	...	Fair	Fine
☾	27	29·85	52	...	62	NW.	0·15	......	1	1	1	...	1	1	...	29·91	29·53	50	58	45	48·5	...	...	Fair	Fine
	28	29·90	48	...	55	N.	...	......	1	...	1	...	1	1	...	30·00	29·70	40	53	42	47·5	...	...	Fair	Fine, hail & rain *p.m.*
	29	30·00	50	...	55	NE.	...	......	...	...	1	1	1	1	...	30·05	29·84	40	57	49	41·5	...	·04	Fair	Fine
	30	29·46	55	...	55	SW.	·15	1·210	1	1	1	...	1	1	1	29·34	29·15	50	50	47	47	...	...	Rain	Rain
Oct.	1	28·70	58	$53\frac{1}{2}$	80	SW.	...	·170	1	1	1	...	...	1	1	28·88	28·80	50	43	42	45·5	...	·61	Rain	Rain
	2	29·36	43	...	76	NW.	...	......	...	...	1	...	1	1	1	29·50	29·15	38	52	40	43	...	1·02	Fair	Rain
	3	29·85	44	...	76	W.	·10	·120	1	1	1	...	1	1	1	29·91	29·60	40	54	49	43	...	·07	Fair	Fine, rain *p.m.*
●	4	29·98	52	...	65	NW.	...	......	1	...	1	...	1	...	...	30·05	29·70	50	58	50	47·5	1·50	·15	Fair	Fine
	5	30·08	58	...	61	SE.	...	......	...	1	1	...	...	...	...	30·10	29·85	50	60	60	50	...	...	Cloudy	Fine
	6	29·90	61	...	72	S.	·12	·135	...	...	1	...	...	...	1	29·88	29·55	58	60	58	59·5	...	...	Rain	Cloudy
	7	29·95	56	$53\frac{1}{2}$	78	W.	...	......	1	1	1	...	1	...	1	30·04	29·65	56	61	45	55	...	...	Fair	Fine
	8	30·10	48	...	75	NW.	...	·270	1	1	1	...	1	...	...	30·05	29·85	47	60	55	44·5	...	...	Fair	Fine
	9	29·60	52	...	59	W.	·18	·250	1	...	1	...	1	1	1	29·61	29·25	50	55	45	48·5	...	·52	Showery	Rain
	10	29·47	47	...	64	SW.	...	·620	1	...	1	...	1	1	1	29·42	29·23	42	48	47	41	...	·03	Stormy	Fine, rain *p.m.* wi. rainb.
	11	28·93	48	...	62	W.	...	·095	1	...	1	...	1	1	1	29·10	28·48	46	56	46	52	·25	·28	Fair	Stormy, rain *a.m.*
☽	12	29·16	46	...	72	W.	·10	·760	1	...	1	...	1	1	1	29·32	29·	44	55	47	46	...	...	Showery	Fine
	13	29·14	53	$53\frac{1}{4}$	68	N.	...	·085	1	...	1	...	1	1	1	29·32	29·15	46	54	40	49·5	...	·24	Showery	Fine, rain *p.m.*
	14	29·44	46	...	72	NW.	...	......	1	1	1	...	1	1	1	29·50	29·20	36	50	42	46	...	...	Rain	Cloudy
	15	29·46	45	...	70	N.	...	·090	1	...	1	...	...	1	1	29·50	29·20	42	54	42	42·5	...	·05	Fair	Cloudy, rain *a.m.*
	16	29·60	48	...	69	W.	·35	·050	1	...	1	...	1	...	1	29·62	29·25	42	53	41	47·5	...	...	Fair	Fine
	17	29·62	46	...	68	N.	...	·050	1	1	1	...	1	1	1	29·62	29·33	41	53	42	43·5	...	...	Fair	Cloudy
	18	29·44	53	...	75	NW.	...	·040	...	...	...	...	...	1	1	29·50	29·28	42	55	50	41·5	·22	...	Cloudy	Fine
○	19	29·58	57	$53\frac{1}{4}$	75	SE.	·15	·030	...	1	1	...	...	...	1	29·70	29·45	52	60	55	50·5	...	...	Fair	Misty
	20	29·90	56	...	72	E.	...	......	1	...	1	...	...	...	...	30·08	29·80	55	60	55	53	...	...	Fair	Cloudy
	21	30·16	54	...	70	NE.	...	......	1	1	1	...	...	...	...	30·25	30·03	51	58	50	50	...	...	Fair	Fine
	22	30·10	51	...	70	E.	·25	......	1	...	1	...	...	...	...	30·15	30·03	50	54	48	50	...	...	Fair	Fine
	23	29·94	49	...	60	E.	...	......	1	...	1	...	...	...	...	30·04	29·90	45	56	48	49	...	...	Fair	Fine
	24	29·95	50	...	60		...	......	1	1	1	...	...	...	...	30·04	29·85	47	56	45	49·5	...	...	Fair	Fine
	25	30·27	51	$53\frac{1}{4}$	66	NE.	·20	......	1	1	1	...	...	...	...	30·25	30·10	40	47	46	45	0·00	...	Cloudy	Misty



Drawn by P. Barlow Jun.r

Eng.d by S. Porter

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

30th NOVEMBER 1823.

LXVI. *An Account of some Electro-magnetic Combinations, for exhibiting Thermo-electric Phænomena, invented by* Mr. JAMES MARSH* *of Woolwich: with Experiments on the same. By* PETER BARLOW, *Esq. F.R.S., of the Royal Military Academy, Honorary Member of the Cambridge Philosophical Society and of the Society of Civil Engineers*†.

PROFESSOR CUMMING having very obligingly favoured me with a copy of his highly interesting paper printed in the Transactions of the Philosophical Society of Cambridge, for the present year, 'On thermo-electric Phænomena,' I put it into the hands Mr. Marsh, requesting him to copy any of the apparatus he there might find described, with a view to repeating such of the experiments as were reported. This he very readily undertook, and soon brought me not only those I had requested him to copy, but several new ones, which latter it is my intention to describe in this paper. The most essential apparatus in these experiments is a very sensible galvanometer. Professor Cumming, who was the first to employ this useful machine‡, has not very distinctly stated his construction of it. I cannot tell, therefore, how nearly that I am about to describe may resemble his; at all events Mr. Marsh's galvanometer is a very simple and sensible instrument, and its construction will not be uninteresting to some of your readers.

AB, CD, (Pl. V.) fig. 1, are two wooden supports or pillars, through which pass brass wires, having each at its extremity a small brass cup; to the other extremity of each is attached by good contact the square helix cage shown in the figure: on the top of the prop E, is a fine point carrying a very light and delicate compass needle with a card below; FG is a stand for holding the bar R S, of bismuth, antimony, or other metal; and *nm n′m′*, are wires of a different metal soldered or bound round at the ends R and S. The set screw at *s* is for adjusting to any height. The brass cups being now

* We are glad to find that this ingenious self-taught artist has commenced the business of chemical and philosophical instrument-maker in Woolwich.—EDIT.

† Communicated by the Author.

‡ See Trans. Camb. Phil. Society for 1821.

rubbed with a little nitrate of mercury, and pure mercury being poured into them, the contact is made or broken at pleasure, by placing the wires in or throwing them out of the cups; and the effect thus produced is shown in the most sensible manner by the needle within the helix; which in very delicate cases may be neutralized by the small magnets *ns*, *ns* in the foot below, when the instrument is placed east and west, or by inserting them in holes, for the purpose, in the props AB, CD, as seen at *p* and *q* when it is in the meridian. Then by applying the lamp at R, then at S, changing the bars and wires, and the size of the latter, &c. all the variety of experiments so judiciously arranged and combined by Professor Cumming may be repeated, and the series extended at pleasure. The galvanometer of Professor Cumming is undoubtedly the same in principle as the above, and may not perhaps differ much in construction; but as he has not described it, the foregoing description will not, I hope, be thought superfluous *.

Apparatus for exhibiting Rotation.

Professor Cumming at the conclusion of his paper has suggested a small combination of platinum and silver wires for exhibiting a rotation about a magnet, as Mr. Faraday had done in the case of a galvanic wire, in his experiments on electro-magnetism, and which certainly threw more light on the inquiry than any experiments before made on the subject.

On constructing a machine precisely from the description given by Professor C., it was found that it had indeed a tendency to revolve, but so small that it was very difficult, if not impossible, to exhibit the phænomenon in a satisfactory manner. It turned out, however, while carrying on the experiment, that, although a magnet in the interior of the wire would not produce any but a weak tendency to rotation, a magnet applied exterior to it was capable of producing the most decided effect, which will be seen as I proceed to describe the following experiments.

For this purpose four rectangles, figures 2, 3, 4, 5, were made of platinum and silver combined, as shown in the figures, where the thicker lines indicate silver wire, and the lighter ones the platinum; a ring being formed below to admit the prop upon which they were to revolve; and a fine steel point brazed to the upper side to rest in the agate on the top of the prop. The stand with a rectangle suspended is shown in figure 6, where AB is a board, *cd* a brass prop with its agate at top, and N S a magnet placed as nearly as possible to the

* Professor Cumming has since described his instrument in the Annals of Philosophy for October last.—Edit.

wire,

wire. The spirit-lamp being now applied at D or E, the rotation will commence, either to the right or left, according to circumstances, and which will be reversed by reversing the pole of the magnet. Fig. 7 shows the same stand with two magnets.

This being premised, the reader will easily follow me in the detail of the following experiments, observing that when the motion is said to be to the right or left, he must imagine himself coinciding in position with the wire about which the machine turns.

Exp. 1. The rectangle, fig. 2, being applied upon the stand, fig. 6, and the lamp at E, the rectangle was projected to the right till D reached the lamp*; it was then propelled back again, and after a few oscillations it remained at rest at right angles to its first position.

Exp. 2. The rectangle adjusted as before, and the lamp applied at D, the wire was projected to the *left* with similar results to the preceding.

Exp. 3. The rectangle adjusted so that D was next the magnet, and the light then applied at D. The wire projected to the *right.*

Exp. 4. The rectangle still in the same position, but the light applied at the other extremity. The wire was projected to the *left.*

Exp. 5, 6, 7, 8, were made under precisely the same circumstances with the rectangle, fig. 3; and the results were similar, but much weaker, and the motions all reversed.

Exp. 9, 10, 11, 12, were made with the rectangle, fig. 4. The motions the same as with rectangle, fig. 2, except that we generally obtained a rotation when the light was applied as in Experiments 2 and 4.

Exp. 13, 14, 15, 16, were still the same experiments, but with the rectangle, fig.5. The results were as in the above case, but reversed in respect to direction, and inferior in force.

A similar set of experiments were made with the south pole of the magnet opposed to the wire, and the results were similar, but all in the reverse direction.

As it was obvious from these results that the rectangle, fig. 4, either from its more accurate balance, or from the nature of the combination, was the most powerful, it was alone made use of in the following experiments, in which two magnets were employed.

Exp. 17. The rectangle, fig. 4, being suspended as shown

* It ought rather to be said *before* D reached the lamp; for when by chance it did reach it, the wire revolved.

 in

in fig. 7, and the lamp applied at E, a rotation immediately commenced to the right, which soon increased to 30 revolutions per minute.

Exp. 18. The rectangle adjusted as before, and the lamp applied at D, the rotation to the left, at the rate of 30 revolutions per minute.

Exp. 19. We now suspended the compound rectangle, fig. 8, and opposed to it the north end of the magnet, as in fig. 9, and applied the lamp successively at E, D, G and F. The following were the results:

Lamp at E, rapid rotation to the right.
Lamp at D, rotation to the left, 30 per minute.
Lamp at G, rotation to the left, ditto.
Lamp at F, no tendency to rotation.

Exp. 20. The magnet reversed, the lamp applied as before.

Lamp at E, rapid rotation to the left.
Lamp at D, rotation to the right, 30 per minute.
Lamp at G, no tendency to rotation.
Lamp at F, rotation to the right, 30 per minute.

Exp. 21. The same experiments were repeated with two magnets with contrary poles opposed, as shown in fig. 10.

The results as above, but more rapid: in the last experiments we obtained about 30 revolutions per minute, but it was impossible to count them in these, when two strong magnets were employed.

It should be observed that in no case could any strong tendency to rotation be observed when a magnet was employed for a support, and the exterior magnet removed: fig. 17, in which one branch is carried further from the magnet, showed the greatest tendency.

A pleasing exhibition of a compound motion of this kind is shown in fig. 16, in which N S is a horse-shoe magnet with an agate at each pole to rest the wire in, and L a lamp which serves to heat each compound rectangle. The motion in this case is not so rapid as in the former, but it is very pleasing, and will continue as long as the lamp burns.

Compound rectangles with six branches (fig. 18) were tried, but there appeared to be little or nothing gained by this increase of number. It appears, I think, that one with four branches performs upon the whole the best. The length of the rectangles is about two inches, the depth an inch; the diameter of the platinum wire $\frac{1}{60}$th of an inch, and that of the silver $\frac{1}{40}$th; but it is by no means necessary to observe these dimensions. In general it may be stated, that the lighter the rectangle, the greater will be its velocity.

Let us now endeavour to trace the theory of these motions.

It

It must already have struck the reader as remarkable, that out of the four positions in which the lamp may be applied in the double rectangle (fig. 9), two of them give a rotation in one direction, and one of them in an opposite one, and that at the fourth point there should be no tendency to rotation whatever, but on the contrary a decided direction; this latter point is to the right of the north pole, or to the left of the south pole of the magnet, the observer assuming his position to coincide with the axis of motion, as already suggested in the preceding part of this article. A satisfactory illustration of this singular phænomenon will, it is presumed, be considered a strong test in favour of the hypothesis on which it is founded, particularly if the same hypothesis should be found competent to the illustration of every other electro-magnetic phænomenon hitherto observed.

In my "Essay on Magnetic Attractions and on Electro-magnetism," I have shown that, by supposing the galvanic conducting wire to act upon the magnet with a tangential force varying inversely as the square of the distance, we may not only illustrate but compute the effect of any proposed combination; let us then see how far this same supposition will assist us in explaining the singular anomaly above mentioned.

The platinum being positive to the silver; when the lamp is applied at the union of the two metals, the fluid will be transmitted through the silver wire under the same circumstances as in a galvanic apparatus, with two plates, it is transmitted through the conducting wire from the copper side to the zinc; it ought therefore, when thus transmitted, to project the north pole of the magnet to the left, the observer now coinciding in position with the wire through which the circuit passes. Or the magnet being fixed, and the wire free as in our case, this latter ought to be projected to the right hand. Thus referring to fig. 9, and conceiving the lamp to be applied at E, the point E ought to be projected to the right hand of the observer, looking towards the magnet, while the points F, D, and G (the circuit in these being descending) ought to be projected to the left, the observer conceiving himself coinciding in position with these respective wires and always looking towards the magnet. On the contrary, when the light is applied at F, then the circuit being ascending at F, and descending in all the other branches, this ought to be projected to the right, and all the others to the left; and so on, for any wire to which the lamp is immediately applied: and of course the contrary to all this ought to happen when the south pole is applied. Let figures 11, 12, 13, 14, represent the four applications of the lamp as stated in experiment 19; and the several

several arrows the direction of rotation as excited by the magnet and lamp in these four cases. Then it is obvious that in fig. 11, answering to the case of the lamp applied at E, the tendencies to rotation are all in one direction, and we ought therefore to expect, as is actually the case, the rotation to be very rapid. In figure 12, two of the forces are in one direction, and two in the other; and therefore if these forces were all equal, we ought to have no motion. But the force at D is very considerable in comparison with that at F or G, both on account of the immediate application of the lamp at E, and the division of the circuit into three branches at O; and the direction of the two latter forces on the respective levers, which being oblique there are necessarily less effective in producing rotation. Again, the force at E is also considerable in consequence of the proximity of the magnet; so that the superior forces at D and E, conspiring in direction, will overpower the other two weak and oblique forces, and produce a very considerable rotation, although inferior to the former.

In fig. 13, the forces at E and G will be the superior forces; and as they conspire in direction, they will overpower the two inferior forces at D and F, which are opposed to them, and a considerable rotation will again ensue.

In fig. 14, the two superior forces also conspire, but with this peculiarity, that the moment the motion ensues, and the arm F arrives at F′ and E at E′, the direction of these two forces no longer assists in giving rotation, being then both in the direction of the radii from the centre, and the resultant acts to bring the machine directly towards the magnet, and thereby to convert the rotation into direction, which the experiment strongly exhibits.

When the south pole of the magnet is opposed to the wire, all these directions of motion will be reversed, and then of course the point of neutralization will be at G, fig. 13: which explains the apparent anomaly of the point of no action being to the right hand of the north pole and to the left of the south pole. In fig. 13 we have seen that with the lamp applied at G the motion is to the left; and when at E, as in fig. 11, it is to the right; it follows, therefore, that between these two there ought to be some position of equilibrium as G′, fig. 13, and where no motion of course ought to ensue. But in this case, instead of the forces at G′ and E′ being directed *from* the centre as in the case of fig. 14, they are directed *to* the centre. So that this state of equilibrium differs from the former in this,—that in the former, the equilibrium is one of *stability*, and in the latter of *instability*, and consequently very difficult to exhibit: in fact, the slightest inclination of the

the flame of the lamp in this situation of it, will give rise to a slow rotation in either direction according to the circumstances of the case. The same principles will enable us to explain the increase of acceleration produced by two magnets with their opposite poles applied as in fig. 7 and fig. 10, and the cause of the non-action of a central magnet, except as in the apparatus of Professor Cumming, where one of the branches is carried further from the centre; in which case a slight tendency to rotation is exhibited equal to the difference of the two opposite forces.

We see thus the marked difference between the electro-magnetic rotations produced by the application of the lamp, as in the cases above, and those produced by the galvanic machine; in the latter case it is essential to have the magnet central, whereas in this the magnet must be exterior to produce the desired effect; and the reason is obvious (referring for example to Experiment X, Essay on Magnetic Attractions), for here, and in all similar cases, the fluid being transmitted from the centre passes down the several branches in the same direction, and is therefore acted upon by the central magnet all in one sense; whereas in these, the fluid being ascending in one branch, and descending in the other, the forces on one side counterbalance those on the other, and the machine remains quiescent.

It has been objected against the hypothesis I have advanced of a tangential force varying inversely as the square of the distance, that if such were the case, the cylinder alluded to above (in Experiment X of my Essay) ought to revolve by the application of an exterior magnet; whereas it is almost entirely insensible to its action. But this objection must fall, if the nature of the forces on the periphery of a circle from an exterior point be properly considered; for let p (fig. 15) be an exterior point, and pa, pb, tangents to the circle abd, then the part of the circumference between a and b will be acted upon in one direction, and all the other part of the circumference in the opposite one; and although the line of action is more considerable in the latter case, the intensity is greater in the former, and the difference between the two is not sufficient to cause the rotation. In other words, the centre of attraction of the periphery of a circle estimated from a point without, falls so near to its centre, that the effect to produce rotation is too weak to render itself sensible.

P.S.—It may be proper to observe, that for the convenience of making the drawings I have represented the magnet as standing upright; but the experiments were generally made with powerful magnets placed horizontally.

LXVII. *On*

LXVII. *On the Caloric of Gases and Vapours, by* M. Poisson; *from the Annales de Chimie, tome* xxiii. *p.* 337: *with Observations by* John Herapath, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THINKING M. Poisson's paper, which you had the goodness to put into my hands, would not be unacceptable to your English readers, I have taken the trouble to translate it from the French, and have added some notes and observations which appeared necessary either to elucidate it or to set its merits in a proper light.

From this paper and those of M. Laplace it is plain with what ardour the subject of gases and heat is pursued on the continent. Would our English philosophers but lend their aid in the securer course of deciding some of the more important and disputed points by experiments, it is manifest we should speedily come to decisive conclusions respecting the nature and laws of heat. In hopes that some of them will shortly take up the complete experimental investigation of so important a question,

I am, gentlemen,

Yours truly,

Cranford, October 15, 1823. J. Herapath.

On the Caloric of Gases and Vapours; by M. Poisson*.

§ I. Let ρ be the density of a gas, θ its temperature in centigrade degrees, and p its elastic force, or its pressure on a unity of surface; then shall we have

$$p = a\,\rho\,(1 + \alpha\,\theta) \qquad (1)$$

α and a being two coefficients of which the first is the same for every gas, and equal to ·00375, and the other should be given for each particular gas. The total quantity of caloric contained in a given weight of gas, a gramme† for instance, we have no method known of computing; but we may consider the excess of this quantity above that which a gramme of the gas contains under a pressure and temperature arbitrarily chosen. Denoting this excess by q, it will become a function

* This paper sets out with a formula drawn from the hypothesis, that the increments of expansion under a constant pressure are proportional to the increments of heat. Other hypothetical views are afterwards introduced to account for particular phænomena, the success of which will best appear in the course of the paper. It may however here be observed, that though this paper does not profess to descend deeply into physical principles, it is nevertheless completely hypothetical; but I shall not stop to point out all the hypothetical parts; I shall merely call the attention of the reader to the more material points.—J. H.

† 15·44579 English grains Troy.

of

of p, ρ, and θ; or, because those variables are connected by the preceding equation, simply a function of p and ρ. Thus we shall have $q = f(p, \rho)$;
f being a function whose form it will be required to determine.

The specific heat of this gramme of gas is the quantity which must be communicated to it to raise its temperature θ one degree; and it will be very nearly $\frac{dq}{d\theta}$. But we may consider this specific heat under two different points of view,—first, in allowing the gas to dilate under an invariable pressure,—and secondly, in keeping the volume constant whilst the temperature and pressure augment together. Hence we shall have in virtue of the first equation

$$\frac{d\varrho}{d\theta} = -\frac{\alpha\varrho}{1+\alpha\theta}, \quad \frac{dp}{d\theta} = \frac{\alpha p}{1+\alpha\theta}.$$

It results, therefore, if we put c for the specific caloric when the pressure is constant, and $c_{,}$ when the volume is constant, that

$$\left.\begin{aligned} c &= -\frac{dq}{d\varrho}\cdot\frac{\alpha\varrho}{1+\alpha\theta}, \\ c_{,} &= \frac{dq}{dp}\cdot\frac{\alpha p}{1+\alpha\theta} \end{aligned}\right\} \qquad (2)$$

which, if we put $\frac{c}{c_{,}} = k$, give

$$\rho\frac{dq}{d\varrho} + kp\frac{dq}{dp} = 0. \qquad (3)$$

It is evident, *a priori*, that this ratio k ought always to exceed unity; for the heat must necessarily be greater to raise the temperature a certain quantity when the gas dilates, than when the density is invariable. Experiment however is the only way of obtaining the value of k, and of discovering to us in what manner it depends on p and ρ. Following the experiments of MM. Gay-Lussac and Welter, cited in the *Mécanique Céleste*, book 12. p. 97, this quantity is sensibly constant for the same gas; and for dry atmospheric air its value is $k = 1{\cdot}375$. Now supposing k independent of p and ρ, the integral of equation (3) is

$$q = f\left(\frac{p^{\frac{1}{k}}}{\varrho}\right);^{*} \qquad (4)$$

* This is a very simple case of the integration of partial differentials. Eliminating $\frac{dq}{d\varrho}$ in the course of integration instead of $\frac{dq}{dp}$ would have given

$$q = f\left(\frac{p}{\varrho^{k}}\right),$$

which is rather closer to the subject and somewhat more easily obtained than M. Poisson's, though in other respects virtually the same.—J. H.

 f being

f being an arbitrary function. From this we obtain

$$p = \rho^{k} \varphi q,$$

and because of equation (1)

$$1 + \alpha \theta = \tfrac{1}{a} \varrho^{k-1} \varphi q;$$

φ being another function. The quantity q remaining the same, if p, ϱ, and θ become p', ϱ', and θ', we shall have

$$p' = \varrho'^{k} \varphi q, \quad 1 + \alpha \theta' = \tfrac{1}{a} \varrho'^{k-1} \varphi q.$$

Eliminating $\varphi\, q$ and observing that $\frac{1}{\alpha} = 266^{\circ}{\cdot}67$ there result

$$\left.\begin{aligned} p' &= p \left(\frac{\varrho'}{\varrho}\right)^{k} \\ \theta' &= (266^{\circ}{\cdot}67 + \theta). \left(\frac{\varrho'}{\varrho}\right)^{k-1} - 266^{\circ}{\cdot}67 \end{aligned}\right\} \qquad (5)$$

These equations (5) comprehend the laws of elasticity and temperature of gases, compressed or dilated without changing their quantity of caloric; such as would take place if the gases were contained in vessels imperviable to caloric*; or when the compression is so rapid, as in the phænomenon of sound, that we may suppose the loss of heat quite insensible. In ignition (*Dans le briquet à air*) for example with air, if the volume is suddenly reduced to a fifth, or if we have $\varrho' = 5\,\varrho$, we find by the preceding value of k

$$\theta' - \theta = 221^{\circ} + {\cdot}83\,\theta;$$

in which it is plain that the augmentation of temperature will be the greater the higher the original temperature θ. For when $\theta = 0$ we have $\theta' = 221^{\circ}$, a temperature which philosophers think sufficient to ignite tinder (*l'amadou*) in compressed air.

Eliminating ϱ in equation (4), by means of equation (1), we have

$$q = f\left\{a\, p^{\frac{1}{k}-1} (1 + \alpha\,\theta)\right\}$$

In order to determine the arbitrary function f, we have need of a new hypothesis. M. Laplace's hypothesis in the 12th book of the *Mécanique Céleste* consists in assuming that the increments of caloric follow the same ratio as those of the temperature, which requires that the function f should be of the first degree with respect to the variable it contains; from which it results that, since $\alpha = \frac{1}{266{\cdot}67}$,

$$q = \mathrm{A} + \mathrm{B}\,(266{\cdot}67 + \theta)\, p^{\frac{1}{k}-1}; \qquad (6)$$

A and B being two arbitrary constants. Whence the specific heats are

$$c = \mathrm{B}\, p^{\frac{1}{k}-1}, \quad c_{,} = \tfrac{1}{k} \mathrm{B}\, p^{\frac{1}{k}-1}$$

They do not therefore depend on the temperature θ, but are

* Such cases I think could never under any circumstances whatever be subjected to experimental examination.—J. H.

known

known for all pressures, when one of them has been determined for one determinate pressure. Following MM. Laroche and Berard, we have $c = \cdot 2669$ for air under a pressure of $^{m}\cdot 76$, the specific heat of an equal weight of water being unity. Calling therefore P the pressure corresponding to the barometric height $^{m}\cdot 76$, we get

$$\cdot 2669 = \mathrm{B}\, \mathrm{P}^{\frac{1}{k}-1};$$

from which we conclude generally

$$c = (\cdot 2669)\left(\frac{\mathrm{P}}{p}\right)^{1-\frac{1}{k}};$$

and the value of $c_{,}$ is deduced from that of c by dividing the latter by k. Since the quantity k exceeds unity, the specific heat of a gramme of air, and generally of any gas whatever, will augment as the elastic force p diminishes.

If we denote by m the quantity of caloric lost by a gramme of air, when its temperature is diminished n degrees, we shall have the pressure p remaining constant,

$$m = n\,(\cdot 2669)\left(\frac{\mathrm{P}}{p}\right)^{1-\frac{1}{k}}$$

For an equal volume, the temperature being invariable, the weight will be $\frac{p'}{p}$ grammes, when the pressure becomes p'. Calling therefore m' the loss of caloric of this other volume for the same diminution of temperature, we get

$$m' = \frac{n\,p'}{p}\,(\cdot 2669)\left(\frac{\mathrm{P}}{p'}\right)^{1-\frac{1}{k}};$$

from which we conclude

$$\frac{m'}{m} = \left(\frac{p'}{p}\right)^{\frac{1}{k}}, * \qquad (7)$$

for the ratio of the quantities of caloric lost by the same volume of air under different pressures.

§ II. The formulæ (6) and (7) are extracted from the 12th book of the *Mécanique Céleste*. M. Laplace has also extended the former to aqueous vapour. For this purpose he supposes, first, that when a gramme of vapour is formed, and neither augmented by more vapour nor diminished by condensation, the ratio of its specific caloric under a constant pressure to its specific caloric under a constant volume is invariable; secondly, that the quantity of caloric necessary to elevate the tempera-

* M. Poisson's formula (7) must be regarded as a mere theoretical conclusion unsupported and even unsanctioned as to numbers by experiments. It is directly at variance with what I have shown, Phil. Mag. vol. lxii. p. 138, follows from M. Laplace's views. What makes it more curious, it is Laplace's own conclusion. Such is the unfortunate inconsistency which follows from the doctrine of caloric even in the hands of such men as Laplace and Poisson.—J. H.

 ture

ture any number of degrees, is proportional to this number, the pressure being constant. This being admitted, if we call C the caloric required to reduce a gramme of water at zero into vapour at 100° and with an elasticity of $^{m}\cdot 76$; Q the caloric necessary to vaporise this same gramme of water and give it a temperature θ, under any pressure p; γ the same specific caloric of the aqueous vapour under the pressure $^{m}\cdot 76$; and finally, if we substitute in equation (6) the barometric altitude h for the pressure p, which it measures, this formula will give $Q = C$ when $h = {}^{m}\cdot 76$ and $\theta = 100°$, and $\frac{d\,q}{d\,\theta} = \gamma$ when $h = {}^{m}\cdot 76$. Determining then in consequence the two arbitrary constants which it contains, it becomes

$$Q = C + \gamma \left\{ (266\cdot 67 + \theta) \left(\frac{m\cdot 76}{h}\right)^{\frac{k-1}{k}} - 366\cdot 67 \right\} \qquad (8)$$

It would be desirable that the accuracy of this formula should be verified by experiment, and the constants C, γ, and k determined with precision.

If we put unity for the specific heat of a gramme of water, or for the quantity of heat necessary to raise its temperature 1°, we shall have $C = 650$ very nearly, by taking the mean of the values found for this quantity by different philosophers. Following MM. Laroche and Berard, we shall likewise have $\gamma = \cdot 847$. Indeed they have not given this value of γ with much confidence; but there is reason to believe it is not far from truth, and we shall therefore adopt it until it be modified by other observations. With respect to the value of k we know of no direct observations by which it can be determined; but an important remark which many philosophers, and particularly MM. Clement and Désormes, have made will enable us to approximate to it.

According to this remark, when a space is saturated with vapour, the quantity of caloric contained in each gramme is sensibly the same whatever be the temperature; so that if for θ in the value of Q we put successively different temperatures, and substitute at the same time for h the corresponding maximum tensions of the vapour, Q will be constant or nearly the same in each case. When $\theta = 100°$, the maximum tension, $h = {}^{m}\cdot 76$, which numbers substituted for θ and h in the value of Q render the coefficient of γ nearly $= 0$. Consequently denoting by H instead of h the maximum tension of any temperature θ, this coefficient of γ must still be nearly $= 0$, whatever be the value of θ. Hence the following approximate equation:

$$(266\cdot 67 + \theta) \left(\frac{m\cdot 76}{H}\right)^{\frac{k-1}{k}} 366\cdot 67 = 0; \qquad (9)$$

from

from which we may determine k by giving to θ any value for which the corresponding one of H has been settled by observation. For example, by the table of M. Biot's *Traité de Physique*, tome 1, p. 531, deduced from the experiments of M. Dalton, $H = {}^{m}\!\cdot\!088742$ when $\theta = 50°$; and therefore the preceding equation gives

$$\frac{k-1}{k} = \cdot 0683 \text{ and } k = 1\cdot 073.$$

By employing values of H corresponding to other values of θ comprised between 0° and 100°, the value of k will scarcely differ from the preceding by a hundredth at most, or a two hundredth at least. We shall therefore retain this value of k, to which joining the preceding values of C and γ our formula (8) becomes

$$Q = 650 + (\cdot 847)\left\{(266\cdot 67 + \theta)\left(\frac{{}^{m}\!\cdot 76}{h}\right)^{\cdot 0683} - 366\cdot 67\right\}. \quad (10)$$

The application of this formula to temperatures far distant from 100° shows us that the quantity Q varies but very little in the case of saturation or when $H = h$. For $\theta = 0°$, we have $H = {}^{mm}5\cdot 059$; whence $Q = 658$. For $\theta = -19°\cdot 59$ M. Gay-Lussac has found $H = {}^{mm}1\cdot 3718$; whence $Q = 662$. When $\theta = 140°$ many philosophers agree in giving to H nearly four times its value at 100°, or four times ${}^{m}\!\cdot 76$; whence we get $Q = 653$. Again, M. Christian makes H nearly twice the last value or eight times ${}^{m}\!\cdot 76$ when $\theta = 170°$; from which Q comes out 661. These values of Q, as we perceive, differ but very little among themselves, though they have ranged over a temperature of nearly 200°, and a tension of vapours from almost nothing to eight atmospheres*. This result shows that k in the case of aqueous vapour is but very little greater than unity; but we cannot, as we have shown above, suppose it precisely equal to unity. We should not forget that Q is not sensibly constant unless when the tension or vapour is a maximum. When

* The evidence in favour of his formula which M. Poisson here adduces in the supposed constancy of Q is illusive. It all results from the high value which Q happens to have. Where would have been the evidence had Q happened to have a much less value? for instance, a value of about 3 or 4 or even 10!! Did probability belong to the views producing this theorem, the coefficient of γ being once nearly = 0 should deviate but very little from it. Its different values even under the range of temperature M. Poisson mentions, have ratios from nothing to infinity. A greater proof of the propriety and justice of my objections cannot be adduced than in the very erroneous values of the tension H immediately following. Nothing, it appears to me, can be a stronger argument of the insufficiency of a theory, than the same formula in one instance coming up nearly to observations, and in another instance closely connected running almost in direct opposition to them.—J. H.

the

the space is not completely saturated, Q, as given by equation (10), will vary more with the variations of h and θ. The specific heat of vapour depends simply on h; for denoting this heat by c we have

$$c = \cdot 847 \left(\frac{^m\cdot 76}{h}\right)^{\cdot 0683}$$

Dividing this by k or 1·073, we have the specific heat under a constant volume.

By means of the value of k we draw from equation (9)

$$H = {}^m\cdot 76 \left(\frac{266\cdot 67 + \theta}{366\cdot 67}\right)^{14\cdot 65}$$

If this equation was correct, that is, if Q was rigorously constant in the case of saturation, this formula would express in this same case the tension of the vapour in terms of the temperature; but though Q varies so little, the preceding value of H wanders in high pressures far too much from observations. Thus when $\theta = 170°$ H comes out 13 atmospheres instead of 8; nor does the formula represent observations but imperfectly in temperatures beneath 100°*.

Whether the vapour be at a maximum or not, equation (1), which is equally applicable to vapours and gases, will always give the density ρ of the vapour when the temperature θ and tension h are known. Therefore calling D the density of the vapour at 100° and under the pressure of $^m\cdot 76$, we obtain

$$\rho = \frac{D\,h}{\cdot 76} \cdot \frac{366\cdot 67}{266\cdot 67 + \theta}.$$

The weight of a litre† of dry air at the temperature of 100° and pressure of $^m\cdot 76$ is equal to $^g\cdot 945$; and the weight of a litre of vapour $\frac{5}{8}$ of it or $^g\cdot 59$. Consequently the weight of a volume v of vapour at the temperature θ and tension h will be

$$\frac{v\,h}{^m\cdot 76} \cdot \frac{187\cdot {}^g 33}{266\cdot 67 + \theta}:$$

the unity volume being the litre. Then calling V the quantity

* In the Annals of Philosophy for December 1821, I have given a theorem which represents experiments within about 2 inches of pressure from 32° to 312° of Fahrenheit, and comprehending a tension from 1-5th inch to 167 inches, or upwards of 5 atmospheres. It indeed seems to agree with the observations much better than they agree with each other. In fact, I am inclined to doubt the correctness of Dr. Ure's experiments in the higher temperatures. From the manner in which he made them, I think the vapour of the mercury must have had considerable influence in augmenting the apparent tensions. Probably Mr. P. Taylor's, in the Phil. Mag., vol. ix. page 452, are nearer the truth, though his not describing the manner of his operating * prevents us from using them with that confidence to which they are very likely entitled.—J. H.

† 61·028 cubic inches, or 2·113 pints.

* We regret that Mr. P. Taylor's absence from home, and pressing engagements, have as yet prevented his communicating through our pages an account of his apparatus. Several of the most eminent men of science both of our own and other countries have examined it.—Edit.

tity of heat necessary to form this quantity of vapour, the water being first at zero temperature, V will be the product of this number of grammes and the quantity Q, given by (10); so that we shall have

$$V = \frac{h\ v}{\cdot 76} \cdot \frac{187\cdot 33}{266\cdot 67+\theta} Q.$$

The unity to which V has respect is the quantity of heat necessary to elevate the temperature of a gramme of water one degree, which, as we know, is 75 times that requisite to liquefy a gramme of ice at zero. Consequently, if we assume this last quantity to be the unity of heat, we must multiply the above value for V by 75.

In steam engines, in which this fluid is employed in a state of saturation, Q does not sensibly vary: the ratio of V to h, or of the quantity of heat usefully employed in pressure on the piston, is then, all other things being alike, reciprocally as $266\cdot 67+\theta$. The higher the temperature θ, therefore, of the vapour, the less will be this ratio; and consequently the expanse of heat will increase less rapidly than the force produced. But the economy which thus results in favour of high pressure engines is far inferior to that which experience seems to indicate; and it is in a less waste of heat, or in other circumstances relative to their construction, that we must look for an explication of the advantage which they present.

§ III. Let us suppose that we have two different gases of the same temperature θ and elasticity p; and whose volumes are v and v'. Were they now put one on the other in a closed vessel of the capacity $v+v'$, it is plain they could preserve an equilibrium, because the temperature is the same and the mutual pressures are equal; but this equilibrium would not be stable. Experience proves that these gases would gradually penetrate each other until they are completely intermixed. It further shows that during this operation heat is neither evolved nor absorbed; so that after a certain time the mixture is perfectly homogeneous; the two gases holding the same proportion in every part, and the temperature and pressure being θ and p. From these facts, established by observation, we may deduce another equally well verified by experience.

If two gases mixed together at the temperature θ fill a volume v; and if p, p' denote the pressures they would separately exert, separately occupying the same volume v, at the same temperature θ, the pressure of the mixture will be $p+p'$. In effect, let us suppose that the two gases at first are distinct, and let $p' 7 p$; then dilating the gas under the pressure p' until p' changes to p, its volume will become

$$\frac{v\,p'}{p},$$

provided

provided the same temperature θ has been preserved. Placing the two gases now one on the other, their united volume is

$$v+\frac{v\,p'}{p} \text{ or } \frac{v}{p}\,(p+p').$$

These gases, according to what we have said above, will equally intermix without changing their temperature or common pressure p. Now by Marriotte's law, which is as true of mixed as of simple gases, if we compress the mixture without changing its temperature until its volume

$$\frac{v}{p}\,(p+p')$$

becomes v, its pressure p will become $p+p'$, the same as we had to prove. Equally good would the principle hold with three or more gases, or with a mixture of gases and vapour; in all cases the united pressure will be equal to the sum of all the pressures which the gases or vapours would singly exert, when separately occupying the same volume v at the same temperature θ. It may be seen in the 12th book of the *Mécanique Céleste* how M. Laplace has deduced this principle from the hypotheses he has made on the caloric and radiation of the gases; we simply propose to exhibit its connexion with another fact which we first announced.

Let n and n' be the number of grammes of two different gases mixed together at the temperature θ under a pressure p and filling a volume v; and let c, c' denote the specific heats of a gramme of these gases under an invariable pressure p, and c'' the specific heat of a gramme of the mixture under the same pressure. Then will

$$(n+n')\,c''=n\,c+n'\,c' \qquad (11)$$

For if we suppose the two gases instead of being mixed merely superposed, so that under the temperature θ and pressure p of the mixture they occupy separate portions u and u' of the total volume v; then, by what we have said above, the quantity of heat will be the same in the two gases thus placed as in the perfect mixture of them. This equality will moreover subsist if we augment by one degree the temperatures of the mixture and of the gases. Now to make this augmentation we must communicate a new quantity $(n+n')\,c''$ of heat to the mixture, and the quantities $n\,c$, $n'\,c'$ to the two gases. The first therefore must be equal to the sum of the other two, which is equation (11)—an equation that may be easily extended to the mixture of any number whatever of gases and vapours. It will give the specific heat of any mixture when that of each of the component gases or vapours is known; and reciprocally we may employ it to find the specific heat of either of the component gases when those of the others and of the mixture are known.

known. Thus MM. Laroche and Berard having determined the specific heat of air mixed with vapour at the temperature 39° and pressure $^{m}\cdot76$, and moreover knowing the number of grammes of dry air and vapour contained in the mixture, as well as the specific heat of dry air under the same pressure $^{m}\cdot76$, have been able to draw from it the specific heat of vapour at the whole pressure $^{m}\cdot76$, and not at the particular tension of the vapour, a case which they have left undecided, *Annales de Chimie*, tome 85, p. 132. This specific heat of vapour is the value of γ, which we have used in the preceding article*.

Our equation (11) will still hold, if for the specific heats c, c', c'', under a constant pressure we substitute the specific heats corresponding under a constant volume. For instance, calling these latter $c_{,}$, $c_{,}'$, $c_{,}''$ we shall have

$$(n+n')\, c_{,}''=n\, c_{,}+n'\, c_{,}'.$$

Let k, k', k'' be the several ratios of c to $c_{,}$, c' to $c_{,}'$, c'' to $c_{,}''$, so that $c=k\, c_{,}$, $c'=k'\, c_{,}'$, $c''=k''\, c_{,}''$, then from equation (11) and the preceding we conclude

$$k''=\frac{n\, k\, c_{,}+n'\, k'\, c_{,}'}{n\, c_{,}''+n'\, c_{,}''};$$

or if the ratios k, k' are unequal, the quantities $c_{,}$, $c_{,}'$ will, according to what we have said in § I., be different powers of the pressure p; from which it results that the ratio k'' will not be independent of p. Thus the ratio of the two specific heats for a constant pressure and volume of the same simple gas being supposed invariable, but different in different gases, cannot be invariable in a mixture of two of more simple gases, or simple gases and vapours. If this ratio has appeared constant in the experiments on atmospheric air of different pressures, it is because the values of the specific heats for the two component gases oxygen and azote are sensibly the same†.

* I cannot satisfy myself of the degree of confidence to be attached to the experiments of MM. Laroche and Berard. Calculations from the influence of currents of air do not impress me with the idea that such methods are susceptible of much accuracy. Besides, it certainly seems to be adverse to the theory of caloric itself, that so rarefied and expanded a body as vapour should have a less specific heat than its generating water; which is the case in the above philosopher's results. Crawford's method is much more simple and direct, and brings out results more favourable to caloric. —J. H.

† M. Poisson seems here to think the atmosphere a mere mechanical mixture of oxygen and azote. Were this the case, the proportion of these elements would scarcely be so uniformly the same in all parts of the atmosphere as philosophers tell us it is. But Mr. Harrop's experiments, namely, that nitrogen confined over water absorbs from it just as much and no more oxygen than is sufficient to make atmospheric air, appear to put it beyond a doubt that the atmosphere is a chemical compound, though perhaps but a weak one.—J. H.

 Supposing

Supposing this ratio constant for vapour as well as for dry air, its value is very different in the two fluids, and cannot therefore be constant in moist air, particularly if the vapour in the air be considerable. Hence the formulæ we have given in § I., being founded on the invariability of the ratio in question, will not apply at the same time to simple gases and mixtures of gases and vapours.

Addition to the preceding Memoir by M. Poisson.

During the printing of this memoir M. Clement has communicated to me the result of a new experiment on the temperature of vapour under a very high pressure. According to this experiment, the pressure of aqueous vapour, in the state of saturation at the temperature of 215° centigrade, is 35 atmospheres. From these data equation (10) gives

$$Q = 659;$$

so that the invariability of the quantity Q appears still to hold good very nearly under this high temperature. Nevertheless, we cannot suppose that the caloric Q is rigorously invariable; for, were this the case, our equation (9) would bring out 54 atmospheres for the *maximum* pressure of vapour at 215°, whereas experiment gives only 35 atmospheres.

LXVIII. *Quadrature of the Circle; and Proportion of the Diameter to the Circumference; containing some Observations on its* Perimeter *and* Area, *tending to demonstrate the utter Impossibility of ever obtaining a perfect Solution of these delusive Problems: together with the true and* only *Cause of the constant Failure of all Attempts to effect it. To which is added, a simple and easy Process of estimating the most useful Properties of the Sphere &c. in Mensuration. By Mr.* John Snart.*

To the Editors of the Philosophical Magazine and Journal.

CONSIDERING the sterling talents and very high capabilities of many of the speculators on these mathematical desiderata, it is truly astonishing that none of them ever appear to have penetrated the *true cause* of their continual disappointment; but, as if they were fully persuaded of the entire practicability of the thing, they have all confidently persevered to obtain a still nearer and nearer approximation,

* Communicated by the Author.

(which

(which could never terminate) until the inscribed and inscribing polygons have been augmented to half a million of sides to lessen their differences. And by the tangent of 30 degrees, or by constantly bisecting the arc, the decimals have been wrought out to 128 places of figures! (a number far too great, either to be used or appreciated), without coming to a *conclusion.* For still the Utopian phantom, like the "*Cube's Duplicature,*" has always eluded their most sanguine grasp, although the imperial largess of Charles the Fifth tempted their efforts with a bait of a hundred thousand crowns! And the States of Holland, at a respectful distance followed the Emperor's example.

The last of these elaborate operations was that of De Lagny, a late mathematician of France, and is one of the many proofs in Nature, that *great learning* is not always competent, nor *needful,* to explain *simple matters;* for had he been *less erudite,* he might probably by dint of *reason alone,* have hit upon, at least the *negation* of, that plain matter of fact at once, by seeing its *impracticability.* Instead of which, his very scientific process only tends to seduce himself and others into the unfathomable abyss of *infinity.*

And yet reflection must needs tell all who use it, that by continually bisecting and comparing the inscribing and inscribed polygons, we only increase the *approximation,* and with it the difficulties: because by bringing them nearer to *equality,* we do but remove the decimal-differences *further off from the separatrix.* And then, as every *finite* number must bear *some* proportion to every other *finite* number, it is but a sophistication of science to expect a *perfeet* conclusion to a process which so obviously leads deeper and more deep at every step, until thought itself is lost and bewildered in the impalpable mazes of *infinitely approximating decimals,* without even the forlorn relief of a *circulate,* or the delusive ground of hope, that a *nonary* instead of a *decary* scale might effect our object and enable us to *mensurate the circle!*

Descending therefore from these sublime heights, let us seek the truth of the matter in the more humble paths of *arithmetic.* Analysing first the functions of those powers with which we would make the comparison.

There are one or two properties of figures, in *mensuration,* which though *not latent,* are notwithstanding pretty much overlooked. That in the *first* power is this: The *first, second* and *third* powers of numbers are not only *lineal, superficial* and *solid,* but they are RECTILINEAR also, in their operations! And unless the integrity of this (and *another*) essential feature be preserved inviolate, they cannot produce their *plenary effect.*

effect. Consequently they can know nothing (until *qualified*) about *bent*, *circular*, or *curved* lines, superficies nor convex solids, such as the perimeters, surfaces and cubic contents of *spherical bodies.*

And as this matter seems heretofore either to have been totally unnoticed or forgotten, perhaps the reader will pardon even a *mechanical demonstration* of this *sine qua non*, which may be given by placing any number of slender rods of known and uniform dimensions *endwise in a row;* letting each be an inch, a foot, &c. long. Suppose, for example, *nine* such of a *foot* long were to be taken; it is very evident that, if placed in a STRAIGHT line, they would extend *nine* feet, as indicated by the *first* power of figures; but should any bend, curve, or angular deflection arise in placing them, it is equally plain that they would fall short of their proper extension, and that the said discrepancy must be proportioned to their aberration from the *right line*. And if numbers were alike subject to these tortuosities, *their* conclusions would be equally indefinite and *defective*; *i. e.* unless their indications have a *plenary* effect, by operating in STRAIGHT lines, their assumptions cannot be *true!* But it is very obvious that figures, as symbols, of whatever they be made the representatives, *must be true;* indeed they are the very and ultimate tests of, *truth itself!* And in this case they would be considered as unities of length, or lineal measure, and therefore the *measured* nine feet must accord with the *numeral* nine feet, or concede the point of infallibility to figures, whose first power it is presumed is herein satisfactorily identified and demonstrated. And with this *rectilineal* power, it is that the mensuration of the *perimeter* of the circle has had to do; a power which is totally incompetent to take cognisance of bent or curved lines, which the segment S of the circle *would be* if the chords thereof were extended to a *thousand millions!* (see fig. 1.) and as we have just seen that figures can know nothing *but straight* lines; these mathematicians measured only the *polygon*, but not the *circle!*

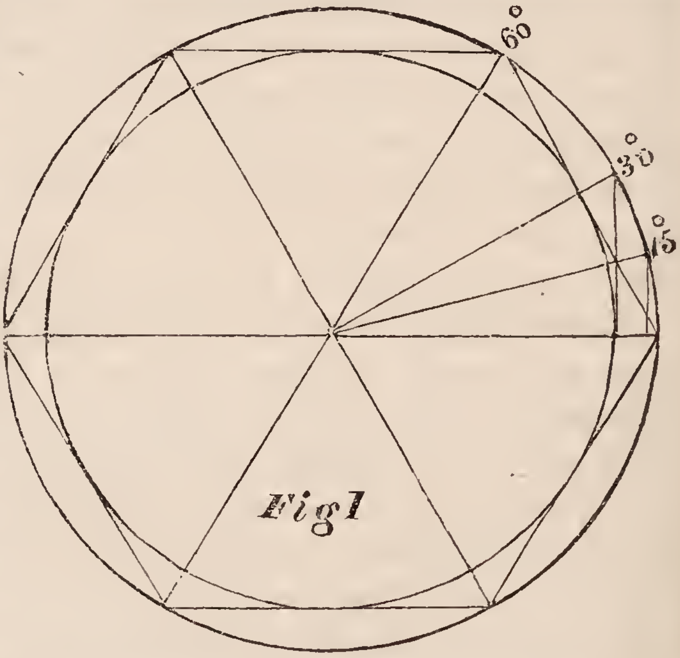
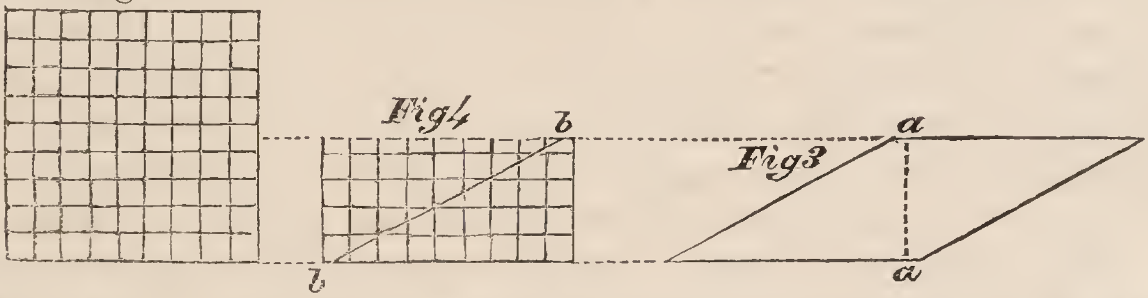


Fig 1

The *second* power of figures, which must also be justified by

by the test of *numbers*, not only generates an area, or superficies; but, to produce a *plenary* effect, as indicated by multiplication, must be *rectangular* as well as *rectilineal*, or else the superficies so generated would be defective in the proportion of such aberration from the *rectangle;* and instead of producing 81 tetragons (fig. 2.), by squaring the nine, we should obtain but 81 lozenges or rhombi (fig. 3,), each of which would be *minus* in proportion to the deflection from the *right angle;* and which, if the declension were carried to 60 degrees from the perpendicular, would sacrifice half the surface, as in fig. 4, producing only 40·5, although the perimeter of the rhombus (fig. 3), whose two segments generated it, was equal to the tetragon (fig. 2). For when that rhombus is bisected in the perpendicular *a a*, and united by the diagonal *b b*, it forms the diminished parallelogram (fig. 4.) Q.E.D.

Fig 2.



The *third*, or *cubic* power of figures, is a compound of the first and second modes, and therefore so similar (indeed *identical*) in its *rectangular* operation, that it is almost superfluous to say any thing about it; as those who are convinced of the sufficiency of the former arguments will scarcely withhold their assent to the latter. For although 729 cubes arise from the primitive root (as $9 \times 9 \times 9 = 729$) when the operations have had their *plenary* or *numerical* effect, yet if the third power of the same root in figures could be alike deflected from the perpendicular, and applied to the oblique angled parallelogram or rhombus above spoken of, the solid product would be only 729 parallelopipeda, *oblique* in all their angles, and whose whole *cubic* content, therefore, would be but 182·25 *solid feet* ($= 729 \div 4$). In which erroneous operation the loss would be no less than 3-4ths of the whole solid mass.

But the integrity of figures cannot be so grossly vitiated, nor is it presumed that their misapplication, in the mensuration of the circle, has been so glaring as the *possibilities* supposed in this *extreme case:* but that their indispensable properties have been invaded and violated, and incompatibilities expected from them, by all those who sought either to *mensurate* or *quadrate* the circle to *perfection* by either of their two first powers, is quite plain.—And that they *did* expect to

to arrive at perfection, is pretty evident, or else they would scarcely have carried the solution to so useless an extent. Indeed Van-eick, the Dutch mathematician, declared that he absolutely *had* effected it; and pertinaciously insisted on the correctness of his construction; and had it not been for the acumen of his cotemporaries in science, who were not so easily convinced, he, without benefiting the world in the least, might have made himself master of the Imperial *douceur*, or 100,000 crowns promised by the Emperor Charles the Fifth! as well as the premium offered by the States of Holland. However, the imperial and princely bounties were both withheld, as unavailing to procure *impossibilities*, and since that time *some* mathematician has declared the thing "*impracticable*, because the proportion is a *surd number*." But thinking an *ipse dixit*, without proof, insufficient to put a stop to this Utopian labour, I have attempted in the simplest manner to demonstrate *why* it is a surd number. How well I have succeeded, must be left to a learned and dispassionate public to determine. However, to make the matter more interesting, I beg leave to subjoin a few words on the nature of the circle, and its measure, as derived from the various polygons.

The difference between the perimeter of the *hexagon* (each of whose six sides is equal to radius) and that of the circle as derived from the sines and tangents of every second of some portion of the quadrant (= to a polygon of 1.296,000 sides; or, if taken from the *sines*, which are always equal to half the chords of double the arcs, = to a polygon of 648,000 sides) is but a trifle less than a *twenty-second* part of the whole circumference thereof, being 16·225323 degrees = $16^\circ\ 13'\ 31''\ 09'''\ 46''''\ 04'''''\ 48''''''$. A difference of nearly one-*seventh* of the whole diameter, and which difference subtracted from $360^\circ\ 00'\ 00''\ 00'''\ 00''''\ 00'''''\ 00''''''$ as measured on the arc, leaves for the triple diameter, or perimeter of the hexagon $= 343{\cdot}774677^\circ = 343^\circ\ 46'\ 28''\ 50'''\ 13''''\ 55'''''\ 12''''''$ of the arc.

Divided by 6, for length of radius on ditto

$57{\cdot}2957795^\circ = 57^\circ\ 17'\ 44''\ 48'''\ 22''''\ 19'''''\ 12''''''$ of the arc.

Length of diameter on ditto

$114{\cdot}591559^\circ = 114^\circ\ 35'\ 29''\ 36'''\ 44''''\ 38'''''\ 34''''''$ of the arc.

The length of the radius (= to the chord of 60 degrees) as herein measured upon the *arc* of the circle, is the quotient arising from dividing 180 degrees by 3·141592653, being a competent part of the great series of 128 figures, or the circumference of a circle whose diameter is *one*, as derived from a polygon so augmented in the number of its sides as to vie with the circle itself.

The other proportions are multiples of that radius by 2 and

and 6, and whose differences are therefore found by subtracting them from 360 degrees.

Then, as the area of any circle is found by multiplying half the *circumference* (in this case $= 1{\cdot}570796325$) by half the *diameter* ($= {\cdot}5$) we obtain ·7853981625 for area of such *circle*, whose $\surd$ ($= {\cdot}886226925$) squared is ·78539816259495562\5, which product may be called a *quadrature* of the circle, as being a *square*, whose area is so nearly equivalent thereto, that it is *perfect* up to the 1000 *millionth* place of figures. And as the thing cannot be done to *absolute perfection*, *discretion* must always adjudge the proper maximum of *approximation* thereto, which may be exemplified by the following

Scholium.

As it has been demonstrated that the circumference of the *circle* can only be mensurated by the sum of the sides and the differences of its greatest inscribing and inscribed *polygons*; so, if those of the inscribed *hexagon* were to be taken, the perimeter would be exactly the same as the *triple diameter*; because each of the six sides thereof is equal to *radius*, or *half the diameter*. Therefore sweeping the outer circle circumscribing the hexagon (fig. 1), any right line from the centre to the circumference may be taken for *radius*; then, without going into abstrusities, we may easily demonstrate, by the extraction of the square root only, any augmentation of the polygon we please; all of whose sides being *rectilineal* are completely mensurable. Thus,

Let radius of the circle, and of the hexagon (either of whose sides, is $=60°$ at the centre), be 1. The sine of half the arc will then be ·5 ($=$sine 30°), the square of which ($= {\cdot}25$) subtracted (by 47th 1st Euclid) from 1· (the square of 1·) $= {\cdot}75$, $\surd\,{\cdot}8660254$ ($=$cosine 30°)$-1{\cdot}0000000 = {\cdot}1339746 =$ versed sine 30° $=$ depth of segment cut off by the chord of any hexagon inscribed in a circle whose radius is *one*.

But if the segments cut off by so simple a figure as that of the hexagon, be *too great* to produce any similitude between that figure and the circle, they become *lessened* by every augmentation of the number of the sides, until *approximation* takes place. Therefore, bisecting the above figure throughout, the *dodecagon* is produced; each of whose 12 sides makes an angle with the centre of 30°. Then, as before. If from the square of radius (1·) be subducted the square of the sine of half the arc (or 15°) $= {\cdot}0669873$, we obtain ·9330127, $\surd\,{\cdot}9659258$ ($=$cosine 15°)$-1{\cdot}0000000 = {\cdot}0340742 =$ versed sine 15° $=$ depth of segment cut off by the chord of any dodecagon inscribed in a circle whose radius is *one*.

Thus,

Thus, by continual bisections, the sides of the polygon may be so augmented as to vie with the perimeter of the *circle itself;* because the segments are *lessened* in the same ratio that the number of sides is increased; but still the *smallest* portions of an *arc* will eternally be *curves*, while the chords of the polygon will as constantly be *straight lines*, and which curves can never be assimilated with a polygon of any definite number of sides whatever.

And had it not been for the sake of the *collateral* branches of science, the *mechanic* might as well have found this approximating proportion between the circumference and the diameter, as the *mathematician*, at least as far as *use* goes, merely by rolling a cylinder of a competent diameter (having a strong spring with a *knife* edge sunk in, but forcing *outward*) over a *true plane of hard surface*, when one revolution would have imprinted a lineal distance between the two light incisions made by the knife's edge to as great a nicety as any *use* could require, which would have done the thing at once, without *deduction;* because the cylinder and plane would have been in *perfect contact* the whole time. But in this enlightened age, and after the thing is done *so well*, it would be Gothic indeed to think of superseding mathematics by a rolling stone.

Therefore, for the benefit of those who are unacquainted with the nature of decimals, and yet have frequent occasion to appreciate these proportions, perhaps a simple modification of the process may be acceptable; especially as it requires no previous science beyond that of the most common school arithmetic, and yet is perfect up to the tenths of millionths, or seventh place of figures.

The circumference of the circle (as shown by the 128 decimals) being about $\frac{1}{7}$ of twice the radius more than the *triple diameter*, may conveniently enough be found by persons of limited arithmetic, by multiplying the given diameter by the very low and comprehensible vulgar fraction of $3\frac{16}{113}$ to produce the *circumference*, which simple fraction will be found much nearer to the truth than the usual *decimal* factor 3·1416. Insomuch that the circumference of the earth (taking its diameter at 7964 miles) is found by this process to be $25019\frac{73}{113}$ miles = 25019·6460, which is nearer the truth of the 128 decimal figures than that given by the usual decimal factor 3·1416 (which gives 25019·7024 miles). This extreme test of the proposed vulgar fraction is a proof that the numerator 16 may safely be used by such unqualified persons as a *constant* multiplier for any accidental diameter; while the denominator 113 is equally correct for the uniform divisor of such multiplied numerator as shall be required by the occasional diameter.

The

The proportion of the *area* of the circle, to that of a tetragon, or *square* whose side or root is equal to that of the circle's diameter, though not so simple, may notwithstanding be worth the notice of those to whom it is addressed. Its area being $\frac{3927}{5000}$ of that of the square, that is, as 3927 is to 5000, or nearly $\frac{11}{14}$ths. So that when any diameter is squared and multiplied by *eleven*, the quotient arising from dividing such product by 14 will be but a trifle *more* than the true area of the circle as found by the quadrating factor ·7854.

The proportion of the *sphere's solidity* in comparison with that of a *cube*, whose side or root is equal to the diameter of such sphere, and whose *decimal* expression of solidity is ·5236 or $\frac{5236}{10000}$, may as well be expressed, for *common* use, by the equivalent vulgar fraction $\frac{1309}{2500}$, or *nearly* so, by the very low fraction of $\frac{11}{21}$; or as 11 is to 21 : : the solidity of the sphere to that of the cube.

Therefore, when any diameter is cubed, or twice involved by its own root, and that product multiplied by 11, the quotient arising from dividing the latter product by 21 will be but a trifle more than that found by the decimal mode. Thus $2 \times 2 \times 2 \times 11 \div 21 = 4{\cdot}190$ instead of 4·1888. That is, by either mode, the solidity of the sphere is but little more than *half* that of the cube of the same depth or thickness.

The *superficies* of any sphere is always equal to *four* times the area of one great circle thereof, and is therefore found by multiplying those areas by 4, or by *squaring* the diameter and multiplying the product thereof by $3\frac{16}{113}$.

Thus, $8 \times 8 \times 3\frac{16}{113} = 201\frac{7}{113}$ ($=201{\cdot}06195+$ by the *decimal* process) = the superficies of a sphere whose diameter is 8.

And $12 \times 12 \times 3\frac{16}{113} = 452\frac{44}{113}$ ($=452{\cdot}38938+$ by the decimal process) = the superficies of a sphere whose diameter is 12. N. B. The numbers indicate the *same kind of measure* as those in which the diameters were first taken, whether feet, inches, or whatever.

How far these little matters may be called *discoveries*, in this enlightened age, when it is difficult to produce any thing *new*, or show any thing that has not been done by others, is a hard matter even for the author himself to determine, unless he were acquainted with *all* that others have done. I shall therefore claim no merit beyond that of an *unpirated* attempt to inform those who most *need it*, and therefore delivered in terms best suited to their *capacities*.

I remain yours, &c.

215 Tooley-street, Oct. 13, 1823. JOHN SNART.

LXIX. *True apparent Right Ascension of Dr.* MASKELYNE'S 36 *Stars for every Day in the Year* 1823, *at the Time of passing the Meridian of Greenwich.* [Continued from page 277.]

1823.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 53	H. M. 4 25	H. M. 5 3	H. M. 5 6	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 30	H. M. 7 34	H. M. 9 18	H. M. 9 59	H. M. 11 40	H. M. 11 41	H. M. 13 15	H. M. 14 7
Dec.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	12·22	18·14	7.28	51·95	44·89	6·90	12·67	40·66	25.14	23·13	6·74	33·90	57·21	0·31	4·92	32·04	55·77	37·79
2	21	13	28	97	91	92	69	67	16	16	77	93	24	35	95	07	80	81
3	20	13	28	98	93	93	71	69	18	19	80	96	27	38	99	10	83	84
4	19	13	28	99	95	94	72	71	20	22	82	99	30	41	5·02	14	86	86
5	18	13	28	52·00	97	96	74	72	22	25	85	34·02	33	45	05	17	89	88
6	17	12	28	01	99	97	76	74	24	28	87	05	36	48	08	20	92	91
7	16	12	28	02	45·01	98	78	76	26	31	90	08	39	51	11	23	95	94
8	15	11	28	03	03	99	79	77	28	34	92	11	42	55	15	26	98	96
9	14	10	28	04	04	7·01	81	79	30	37	94	14	45	58	18	30	56·01	99
10	13	10	28	05	06	02	82	80	32	40	97	17	48	61	22	33	04	38·02
11	12	09	28	06	07	03	84	82	34	42	99	19	52	65	25	36	07	04
12	11	09	28	06	09	04	85	83	35	45	7·01	22	55	68	28	40	10	07
13	10	08	28	07	10	04	86	85	37	48	03	25	58	71	32	43	13	10
14	09	07	28	08	11	05	88	86	39	51	06	28	61	74	35	46	16	12
15	08	07	27	08	13	06	89	88	40	53	08	30	64	78	39	50	19	15
16	07	06	27	09	14	07	90	89	42	56	10	33	67	81	42	53	22	18
17	06	05	27	10	15	08	91	91	44	59	12	36	70	84	45	56	25	21
18	05	05	26	10	16	09	92	92	45	61	14	38	73	87	49	60	28	24
19	04	04	26	11	17	09	93	93	47	63	17	40	75	91	52	63	32	26
20	03	03	26	11	18	10	94	94	48	66	19	43	78	94	56	66	35	29
21	01	02	25	12	19	10	95	95	50	68	21	45	81	97	59	70	38	32
22	00	02	25	12	20	11	96	96	51	71	23	48	83	1·00	62	73	42	35
23	11·99	01	24	12	21	11	97	97	52	73	25	50	86	03	66	76	45	38
24	98	00	24	13	22	11	98	98	54	75	27	52	88	06	69	80	48	41
25	97	17·99	23	13	22	12	98	99	55	78	29	55	91	09	73	83	52	44
26	96	98	23	13	23	12	99	41·00	56	80	31	57	93	12	76	87	55	47
27	95	97	22	13	24	12	13·00	01	57	82	33	59	96	15	79	91	58	50
28	94	96	22	13	25	13	00	02	58	84	35	61	98	18	83	94	62	53

Dec.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	57·68	9·10	13·58	35·74	37·25	36·83	45·32	57·75	53·21	11·41	39·83	53·15	16·93	25·45	44·96	55·32	0·79	19·64
2	71	13	60	76	27	84	33	75	20	41	82	14	92	43	95	30	78	63
3	73	15	62	78	29	85	33	74	20	40	82	14	92	41	94	29	77	62
4	75	17	63	79	30	85	34	73	19	39	81	13	91	39	93	28	75	61
5	77	19	65	81	32	86	34	73	18	39	81	13	91	37	92	26	74	59
6	80	22	67	83	34	87	35	72	18	38	80	12	90	35	91	25	73	58
7	83	25	69	85	36	88	36	72	17	38	79	11	89	33	90	24	72	57
8	85	27	71	87	38	89	36	72	17	37	79	11	89	31	89	22	71	56
9	88	30	73	89	40	90	37	72	17	37	79	10	88	30	88	21	70	54
10	90	32	74	90	41	91	38	71	16	37	79	10	88	28	87	19	69	53
11	93	35	76	92	43	92	39	71	16	36	78	10	88	26	86	18	67	52
12	96	38	78	94	45	93	40	71	16	36	78	09	87	25	85	17	66	50
13	98	40	80	96	47	94	41	71	16	36	78	09	87	23	84	15	65	49
14	58·01	43	83	99	50	95	42	71	15	36	78	09	87	22	83	14	64	48
15	04	46	85	36·01	52	97	43	70	15	36	78	08	86	20	82	12	63	46
16	07	49	87	03	54	98	44	70	15	36	78	08	86	19	81	11	62	45
17	10	52	89	05	56	99	45	70	15	36	78	08	86	17	80	10	61	44
18	13	55	92	08	59	37.01	47	70	15	36	78	08	86	16	79	08	60	42
19	16	58	94	10	61	02	48	71	15	36	78	08	86	15	78	07	59	41
20	19	61	96	12	63	04	49	71	15	36	78	08	86	14	78	06	58	39
21	22	64	99	15	66	05	51	71	15	36	78	08	86	12	77	05	56	38
22	25	67	14·01	17	68	07	52	72	15	36	78	08	86	11	76	04	55	37
23	28	70	04	19	70	09	53	72	15	36	78	08	86	10	76	02	54	35
24	31	73	06	22	73	10	55	73	15	36	78	08	86	09	75	01	53	34
25	34	76	08	24	75	12	56	74	16	36	78	08	86	08	75	00	52	32
26	37	79	11	27	78	14	58	74	16	36	78	08	86	07	74	54·99	51	31
27	40	82	14	30	81	16	60	75	16	36	79	08	86	06	74	98	50	30
28	43	85	16	32	83	18	61	76	17	37	79	09	87	06	73	96	49	28
29	47	89	19	35	86	20	63	77	17	37	79	09	87	05	73	95	48	27
30	50	92	22	38	88	22	64	78	18	37	80	09	87	04	72	94	47	26
31	53	95	25	40	91	24	66	79	18	38	80	10	88	04	72	93	46	25

N.B. On those days where an Asterisk is prefixed the Star passes twice; the Æ given is that at the first passage.

LXX. *Derivative Analysis; being a new and more comprehensive Method of the Transformation of Functions than any hitherto discovered: extending not only to the Extraction of the Roots of Equations, but also to the Reduction of Quantities from the Multiples of Powers or Products to other equivalent Expressions, by which the Summation of any rational Series may be readily effected. By Mr.* PETER NICHOLSON.

5 Claremont-place, Judd-street.

[Continued from p. 252.]

Ex. 7. DIVIDE the series $C+\frac{B}{v}+\frac{B_2}{v^2}+\frac{C_2}{v^3}+\frac{D_2}{v^4}+$ &c. (which is the quotient of the preceding example increased by the quantity C) by the binomial $v-e$ (which is the same divisor as in the two preceding examples).

$$\begin{array}{l|l} & \text{Divisor.} \\ C+\frac{B}{v}+\frac{B_2}{v^2}+\frac{C_2}{v^3}+\frac{D_2}{v^4}+\&c. & v-e \\ & \text{Quotient} \\ C-\frac{eC}{v} & \frac{C}{v}+\frac{B_3}{v^2}+\frac{C_3}{v^3}+\frac{D_3}{v^4}+\&c. \\ \hline \quad\frac{B_3}{v}+\frac{B_2}{v^2} & \\ \quad\frac{B_3}{v}+\frac{eB_3}{v^2} & \\ \hline \qquad\frac{C_3}{v^2}+\frac{C_2}{v^3} & \\ \qquad\frac{C_3}{v^2}-\frac{eC_3}{v^3} & \\ \hline \qquad\quad\frac{D_3}{v^3}+\frac{D_2}{v^4} & \\ \qquad\quad\&c. & \end{array}$$

By this operation we have the following derivative equations,

$$B_3 = eC$$
$$C_3 = eB_3 + B_2$$
$$D_3 = eC_3 + C_2$$
&c.

Problem.

To divide a quantity by a new divisor at each step equal to that in the preceding step.

Find the first part of the quotient and the remainder by the first divisor.

Find the second part of the quotient and the second remainder by the second divisor.

Proceed from step to step in this manner as far as may be judged necessary, always substituting at each step as directed.

The several equations being tabulated, will show the law by which

which the coefficients of the quotient may be derived from each other.

Examples.

Ex. 1. Divide the quantities A by $r-a'=s-b'=t-c'=u-d$ &c.

Operation.

$$\begin{array}{l|l} & \text{Divisors.} \\ A \ldots\ldots & r-a'=s-b'=t-c'=u-d'=\&c. \\ A-\dfrac{a'A}{r} & \text{Quotient} \\ \hline \dfrac{B_1}{r} & \dfrac{A}{r}+\dfrac{B_1}{rs}+\dfrac{C_1}{rst}+\dfrac{D_1}{rstu}+\&c. \end{array}$$

$$\begin{array}{l} \dfrac{B_1}{r}-\dfrac{b'B_1}{rs} \\ \hline \dfrac{C_1}{rs} \\ \dfrac{C_1}{rs}-\dfrac{c'C_1}{rst} \\ \hline \dfrac{D_1}{rst} \\ \&c. \end{array}$$

From the operation herewith executed we have the following derivative equations, viz.

$$B_1=a'A$$
$$C_1=b'B_1$$
$$D_1=c'C_1$$
$$\&c.$$

Ex. 2. Divide $B+\frac{A}{r}+\frac{B_1}{rs}+\frac{C_1}{rst}+$&c. (which is the quotient of the preceding example increased by the quantity B) by $r-a''=s-b''=t-c''=u-d''$ &c.

$$\begin{array}{l|l} B+\dfrac{A}{r}+\dfrac{B_1}{rs}+\dfrac{C_1}{rst}+\&c. & r-a''=s-b''=t-c''=u-d''=\&c. \\ B-\dfrac{a''B}{r} & \dfrac{B}{r}+\dfrac{B_2}{rs}+\dfrac{C_2}{rst}+\dfrac{D_2}{rstu}+\&c. \\ \hline \dfrac{B_2}{r}+\dfrac{B_1}{rs} & \end{array}$$

$$\begin{array}{l} \dfrac{B_2}{r}-\dfrac{b''B_2}{rs} \\ \hline \dfrac{C_2}{rs}+\dfrac{C_1}{rst} \\ \dfrac{C_2}{rs}-\dfrac{c''C_2}{rst} \\ \hline \dfrac{D_2}{rst}+\&c. \\ \&c. \end{array}$$

From

From the operation now executed we have the following derivative equations,

$$\begin{aligned} B_2 &= a''B + A \\ C_2 &= b''B_2 + B_1 \\ D_2 &= c''C_2 + C_1 \\ &\text{\&c.} \quad \text{\&c.} \end{aligned}$$

Whence the law of derivation is obvious.

Ex. 3. Divide the series $C + \frac{B}{r} + \frac{B_2}{rs} + \frac{C_2}{rst} + \frac{D_2}{rstu} + \text{\&c.}$ (which is the quotient of the preceding example increased by the quantity C) by $r-a''=s-b''=t-c''=u-d''=\text{\&c.}$

$$\begin{array}{l|l} & \text{Divisor.} \\ C + \frac{B}{r} + \frac{B_2}{rs} + \frac{C_2}{rst} + \frac{D_2}{rstu} + \text{\&c.} & r-a'''=s-b'''=t-c'''=u-d'''=\text{\&c.} \\ C - \frac{a'''C}{r} & \text{Quotient} \\ \hline \quad \frac{B_3}{r} + \frac{B_2}{rs} & \frac{C}{r} + \frac{B_3}{rs} + \frac{C_3}{rst} + \frac{D_3}{rstu} + \text{\&c.} \\ \quad \frac{B_3}{r} - \frac{b'''B_3}{rs} & \\ \hline \qquad \frac{C_3}{rs} + \frac{C_2}{rst} & \\ \qquad \frac{C_3}{rs} - \frac{c'''C_3}{rst} & \\ \hline \qquad\qquad \frac{D_3}{rst} & \\ \qquad\qquad \text{\&c.} & \end{array}$$

From the operation now executed we have the following derivative equations

$$\begin{aligned} B_3 &= a'''C + B \\ C_3 &= b'''B_3 + B_2 \\ D_3 &= c'''C_3 + C_2 \\ &\text{\&c.} \end{aligned}$$

Let $Avwx$ &c. + Bwx &c. + Cx &c. + D &c. + &c. be any function of u, and $Av'w'x'$ &c. + $B_n v'w'$ &c. + $C_n v'$ &c. + D_n &c. + &c. be another function of u equal to the former, then will

$$\begin{array}{l|l|l} B_1 = B + (v-v')A & C_2 = C + (w-v')B_1 & D_3 = D + (x-v')C_2 \\ B_2 = B_1 + (w-w')A & C_3 = C_2 + (x-w')B_2 & \quad \text{\&c.} \\ B_3 = B_2 + (x-x')A & \quad \text{\&c.} & \\ \quad \text{\&c.} & & \end{array}$$

And finally, let n be the greatest number of factors; and if a_n be the difference between the nth factors of the first term of each series, b_n the difference between the $n-1$th factors in the second terms; and so on, then will

$B_n =$

$B_n = B_{n-1} + a_n A$, $C_n = C_{n-1} + b_n B_{n-1}$, $D_n = D_{n-1} + c_n C_{n-1}$ &c.

For let $v - v' = a_1$,

$w - w' = a_2$, $w - v' = b_2$,

$x - x' = a_3$, $x - w' = b_3$, $x - v' = c_3$,

&c.

Then by transposition

$v = v' + a_1$

$w = w' + a_2 = v' + b_2$

$x = x' + a_3 = w' + b_3 = v' + c_3$

&c.

$$A = A \tag{1}$$

Now since $A = A$

Multiply the first side of this equation by v, add B to the product and multiply the second side by $v' + a_1$ and add B to the product, and

$$Av + B = \begin{cases} Av' + B \\ \quad\; + a_1 A \end{cases} \tag{2}$$

$$Av + B = Av' + B_1 \tag{3}$$

Let $Av + B = Av' + B_1$

Multiply the first side of this equation by w, add C to the product, and of the two terms on the second side multiply the first by $w' + a_2$, the second by $v' + b_2$, and add C to the product, and

$$Avw + Bw + C = \begin{cases} Av'w' + B_1 v' + C \\ \quad\; + a_2 A\, v' + b_2 B_1 \end{cases} \tag{4}$$

$$Avw + Bw + C = Av'w' + B_2 v' + C_2 \tag{5}$$

Let $Avw + Bw + C = Av'w' + B_2 v' + C_2$

Multiply the first side of this equation by x, add D to the product, and of the three terms on the second side multiply the first by $x' + a_3$, the second by $w' + b_3$, the third by $v' + c_3$, and add D to the product, and

$$Avwx + Bwx + Cx + D = \begin{cases} Av'w'x' + B_2 v'w' + C_2 v' + D \\ \quad\; + a_3 A\, v'w' + b_3 B_2 v' + c_3 C_2 \end{cases} \tag{6}$$

$$Avwx + Bwx + Cx + D = Av'w'x' + B_3 v'w' + C_3 v' + D_3 \tag{7}$$

Let $Avwx + Bwx + Cx + D = Av'w'x' + B_3 v'w' + C_3 v' + D_3$

&c. &c.

Now, by comparing the coefficients of the corresponding terms of the second sides of equations 2 and 3 will be found

$$B_1 = B + a_1 A.$$

And by comparing the coefficients of the corresponding term of the second sides of equations 4 and 5 will be found

$$B_2 = B_1 + a_2 A, \quad C_2 = C + b_2 B_1.$$

And

And by comparing the coefficients of the corresponding terms of the second sides of equations 6 and 7 will be found

$$B_3=B_2+a_3A,\ C_3=C_2+b_3B_2,\ D_3=D+c_3C_2.$$

Let now $v=u+a,\ w=u+b,\ x=u+c$ &c.

Also let $v'=u+\alpha,\ w'=u+\beta,\ x'=u+\gamma$ &c.

Then will
$$\begin{array}{lll} a_1=a-\alpha, & b_2=b-\alpha, & c_3=c-\alpha \\ a_2=b-\beta, & b_3=c-\beta & \&c. \\ a_3=c-\gamma, & \&c. & \\ \&c. & & \end{array}$$

Whence

$$\begin{array}{l|l|l} B_1=B+(a-\alpha)A & C_2=C+(b-\alpha)B_1 & D_3=D+(c-\alpha)C_2 \\ B_2=B_1+(b-\beta)A & C_3=C_2+(c-\beta)B_2 & \&c. \\ B_3=B_2+(c-\gamma)A & \&c. & \\ \&c. & & \end{array}$$

A very convenient form for the arithmetical operation is as follows:

For a simple function,

$$\begin{array}{l|l} & B \\ \hline (a-\alpha) & B_1 \end{array}$$

For a quadratic function,

$$\begin{array}{l|l|l} & B & C \\ \hline (a-\alpha)\ A & B_1\times(b-\alpha)=Q_1 & C_1 \\ (b-\beta,\ A & B_2 & \end{array}$$

For a cubic function,

$$\begin{array}{l|l|l|l} & B & C & D \\ \hline (a-\alpha)\ A & B_1\times(b-\alpha)=Q_1 & C_2\times(c-\alpha)=R_1 & D_3 \\ (b-\beta)\ A & B_2\times(c-\beta)=Q_2 & C_3 & \\ (c-\gamma)\ A & B_3\quad \&c. & & \end{array}$$

Explanation.

$B+(a-\alpha)$ gives B_1, and $B_1\times(b-\alpha)$ gives Q_1, $C+Q_1$ gives C_2

$B_1+(b-\beta)$ ...... B_2 ... $B_2\times(c-\beta)$ ... Q_2, C_2+Q_2, ... C_3

$B_2+(c-\gamma)$ ...... B_3 &c.

&c.

The rule thus exhibited may easily be expressed in words at length as follows:

Place the coefficients of the given function in a horizontal line.

From the first, second, third, &c. factors of the first term of the given function, subtract the first, second, third, &c. factors of the first term of the function of which the coefficients are required each from each.

Multiply the coefficient of the first term of the given function by each of the differences, and write the products in a column under the second coefficient.

Write

Write the successive sum of the coefficient of the second term of the given function and each product in another column on the right, and the last of these successive sums opposite the last product is the coefficient of the second term of the transformed function.

From the first, second, &c. factors of the second term of the given function subtract the first, second, &c. factors of the second term of the function of which the coefficients are required each from each.

Multiply each of the successive sums by each of the differences of the factors of the second terms, and place the products in a column under the coefficient of the third term of the given function.

Write the successive sums of the coefficient of the third term and the products in a column on the right, and the last of these successive sums is the coefficient of the third term of the transformed function.

Proceed in the same manner to the last coefficient, which being added to the product under it, the sum will be the absolute number of the transformed function.

Transform the cubic function of binomial factors

$$(x+5)(x-3)(x+2)+3(x-3)(x+2)-5(x+2)+6 \text{ into}$$
$$(x+3)(x+2)(x-5)+B_3(x+3)(x+2)+C_3(x+3)+D_3$$

$$\begin{array}{r|rrr} +3 & +5 \,\vdots & -3 \,\vdots & +2 \\ +2 & -3 \,\vdots & +2 \,\vdots & \\ -5 & +2 \,\vdots & & \end{array} \qquad \begin{array}{r|l|l|l} & \quad 3 & \quad -5 & \quad 6 \\ \hline +2 & +5\times-6=-30 & -35\times-1=35 & 41=D_3 \\ -5 & \pm 0\times\pm 0=\pm\ 0 & -35=C_3 & \\ +7 & +7=B_3 & & \\ * & & & \end{array}$$

Whence

$(x+5)(x-3)(x+2)+3(x-3)(x+2)-5(x+2)+6$ is transformed to

$$(x+3)(x+2)(x-5)+7(x+3)(x+2)-35(x+3)+41$$

Transform $(x+2)(x-3)(x+4)(x-2)$ in terms of the powers of x. That is, to transform

$$(x+2)(x-3)(x+4)(x-2)+0(x+3)(x+4)(x-2)+0(x+4)(x-2)+0(x-2)+0 \text{ into}$$
$$(x+0)(x+0)(x+0((x+0)+B_4(x+0)(x+0)(x+0)+C_4(x+0)(x+0)+(D_4(x+0)+E_4$$

$$\begin{array}{r|rrrr} 0 & +2 \,\vdots & -3 \,\vdots & +4 \,\vdots & -2 \\ 0 & -3 \,\vdots & +4 \,\vdots & -2 \,\vdots & \\ 0 & +4 \,\vdots & -2 \,\vdots & & \\ 0 & -2 \,\vdots & & & \end{array}$$

* The differences above are found mentally, as 5 minus 3 is $+2$, and -3 minus 2 is -5, and 2 minus -5 is $+7$; these are the respective differences of the factors in the first terms. Again, -3 minus $+3$ is -7, and $+2$ minus $+2$ is 0; these are the differences of the factors of the second terms. Again, $+2$ minus $+3$ is -1, which is the difference of the factors in the third term.

Sub-

Subtract as many of the first column as are opposite to each of the other columns, from each of these other columns will give each respective column of differences.

Here $A=1$, $B=0$, $C=0$ &c.

0	0	0	0	
$1\times+2=+2$	$+2\times-3=-6$	$-6\times+4=-24$	$-24\times-2=48$	$48=E_4$
$1\times-3=-3$	$-1\times+4=-4$	$-10\times-2=+20$	$-4=D_4$	
$1\times+4=+4$	$+3\times-2=-6$	$-16=C_4$		
$1\times-2=-2$	$+1=B^{1}$			

Whence $(x+2)(x-3)(x+4)(x-2)=x^4+x^3-16x^2-4x+48$

Transform $3(x+6)(x+1)(x+4)(x-3)$ into the series

$3x(x+1)(x+2)(x+3)+B_4x(x+1)(x+2)+C_4x(x+1)+D_4x+E_4$

0	$+6$	$+1$	$+4$	-3
1	$+1$	$+4$	-3	
2	$+4$	-3		
3	-3			

0	0	0	0	
$3\times+6=+18$	$18\times+1=+18$	$+18\times+4=+72$	$+72\times-3=-216$	$-216=E_4$
$3\times\pm0=\pm0$	$18\times+3=+54$	$+72\times-4=-288$	$-216=D_4$	
$3\times\mp2=\mp6$	$24\times-5=-120$	$-48=C_4$		
$3\times-6=-18$	$6=B_4$			

Whence

$3(x+6)(x+1)(x+4)(x-3)$ is transformed to the series

$3x(x+1)(x+2)(x+3)+6x(x+1)(x+2)-48x(x+1)-216x-216$

Transform the series $5x^4-2x^3+5x^2-4x+6$ into the series $5(x+1)(x+2)(x+3)(x+4)+B_4(x+1)(x+2)(x+3)+C_4(x+1)(x+2)+D_4(x+1)+E_4$

1	0	0	0	0
2	0	0	0	
3	0	0		
4	0			

-2	$+5$	-4	6	
$5\times-1=-5$	$-7\times-1=7$	$12\times-1=-12$	$-16\times-1=16$	$22=E_4$
$5\times-2=-10$	$-17\times-2=34$	$46\times-2=-92$	$-108=D_4$	
$5\times-3=-15$	$-32\times-3=96$	$142=C_4$		
$5\times-4=-20$	$-52=B$			

Whence

$5x^4-2x^3+5x^2-4x+6$ is transformed to the series

$$5(x+1)(x+2)(x+3)(x+4)-52(x+1)(x+2)(x+3)+142(x+1)(x+2)-108(x+1)+22.$$

Let it be required to transform the biquadratic function $x^4+3x^3+2x^2+4x+5$ to the form $x(x+1)(x+2)(x+3)+B_4x(x+1)(x+2)+C_4x(x+1)+D_4x+E_4$.

Here $A=0$, $B=3$, $C=2$, $D=4$, and $E=5$.

	3	2	4	5
0	$3\times-0=\ 0$	$2\times-0=0$	$4\times 0=0$	5
-1	$2\times-1=-2$	$0\times-1=0$	4	
-2	$0\times-2=\ 0$	0		
-3	-3			

Therefore the proposed function is transformed to

$$x(x+1)(x+2)(x+3)-3x(x+1)(x+2)+4x+5;$$

or more shortly expressed to $x^{4|1}-3x^{3|1}+4x+5$; and consequently $x^4+3x^3+2x^2+4x+5=x^{4|1}-3x^{3|1}+4x\quad 5$.

And hence, if x be considered as the number of the term of a series, and if $x^4+3x^3+2x^2+4x+5$ be considered as the general term, the sum of x terms will be $\frac{x^{5|1}}{5}-3\frac{x^{4|1}}{4}+2x^{2|1}+5x$, or in full length $\frac{x(x+1)(x+2)(x+3)(x+4)}{5}-\frac{3}{4}x(x+1)(x+2)(x+3)+2x(x+1)+5x$.

[To be continued.]

LXXI. *An Examination of certain Minerals. By* Augustus Arfwedson*.

Cinnamon-stone of Malsjö.

PROFESSOR Berzelius, while on a mineralogical journey in Wermeland, in the summer of 1822, met with a garnet-like fossil, in the lime-quarry of Malsjö in the vicinity of Philippstadt, which bore a great similarity in its external character to the cinnamon-stone of Ceylon, and likewise exhibited similar properties before the blowpipe. By the following analytical examination, I hope to prove that both fossils, even in their component parts, strongly resemble each other.

This stone is not affected by concentrated muriatic acid, at common temperatures at least; its adhering calcareous envelope is merely dissolved. 1,526 grammes of it, purified in this manner, reduced to powder, and afterwards levigated and dried, were mixed with thrice as much subcarbonate of potassa and exposed to a red-heat. The fused grayish-green mass was dissolved in muriatic acid, and a portion of silica remained, which, after ignition, weighed 0,625 gram. (a). With the usual precaution, in order to prevent the alumina from redissolving, the solution was precipitated with caustic ammonia, after which the apparently ferriferous precipitate was placed on a filter, and

* This paper was originally published in the Transactions of the Royal Academy of Sciences of Stockholm, for 1822; it is given above from a German translation in Schweigger and Meinecke's *Neues Journal*, band viii. p. 1.

 washed

washed with boiling water. It was again dissolved in muriatic acid and caustic potassa added to excess: the precipitate became redissolved, and left oxide of iron behind, which, after exposure to a red-heat, weighed 0,067. On redissolving it in muriatic acid, it was found to contain 0,007 gram. (*b*) of silica; thus there remained 0,06 (*c*) for the oxide of iron. The alumina was separated from the alkaline fluid by muriatic acid and carbonate of ammonia, and weighed 0,321 gram. This also left, after solution in sulphuric acid, 0,007 gram. (*d*) of silica. The quantity of alumina was therefore 0,314 gramm. (*e*)

The fluid, precipitated with caustic ammonia, and then completely neutralized by a few drops of muriatic acid, was treated with oxalate of ammonia, which separated oxalate of lime. This was well washed with warm water, exposed to a red-heat, mixed with a little carbonate of ammonia, and gently heated in order to drive off the excess of ammonia; in this manner 0,920 gram. of carbonate of lime were obtained, equivalent to 0,518 of pure lime (*f*).

The liquid, freed from lime, was mixed with a sufficient portion of subcarbonate of potassa, and evaporated to dryness. This dried mass, after solution in water, left a substance behind, which, after being ignited, weighed 0,006 gram. and, upon trial, proved to be oxide of manganese with a trace of magnesia (*g*).

A portion of this fossil coarsely powdered, and exposed to a red-heat in a platinum crucible, suffered no loss of weight.

This fossil has therefore yielded,

		In 100 Parts.	Oxygen.
Silica (*a*) 0,625			
(*b*) 0,007			
(*d*) 0,007	0,639	41,87	21,06
Alumina (*e*)	0,314	20,57	9,06
Lime (*f*)	0,518	33,94	9,53
Oxide of iron (*c*)	0,060	3,93	1,20
Oxide of Manganese, and Magnesia	0,006	0,39	
	1,537	100,70	

From this it appears, that the proportion of oxygen in the silica is equal to that of the bases taken together; and that, besides this, the quantity of oxygen in the alumina and in the lime is equal; and is, in each of these bases, eight times as great as in the protoxide of iron. The formula, therefore, which expresses the composition of this fossil will stand thus,

FS +

FS+8 AS+8 CS. Klaproth's analysis of the Ceylon cinnamon-stone yielded,

Silica...............	38,80
Alumina	21,20
Lime	31,25
Oxide of iron	6,50
Loss	2,25

This result indeed does not differ considerably from my own; but with regard to the smallest constituent part, viz. the oxide of iron, it gives an essentially different formula; for it becomes FS+4 CS+5 AS. It may be apprehended, that Klaproth obtained too little, as well of the lime as of the alumina, because he separated the first by carbonate of soda, and the latter, from its solution in potassa, by muriate of ammonia. His formula may therefore be considerably faulty in this respect. I think there is reason to consider, from my analysis, that this fossil from Malsjö is a real cinnamon-stone; until, at least, Klaproth's analysis be repeated and its accuracy confirmed.

Brazilian Chrysoberyl.

Our knowledge of the component parts of this mineral is derived from Klaproth's analysis, according to which 100 parts are stated to contain,

Alumina	71,50
Lime	6,00
Oxide of iron ...	1,50
Silica...............	18,00

From a series of trials before the blowpipe, to which Professor Berzelius has subjected most mineral productions, he supposes that this fossil cannot essentially contain lime, but that, according to all circumstances, chrysoberyl is a pure bisilicate of alumina. I am enabled to confirm this supposition by the analytical examination which I here communicate.

Analysis.

0,214 gram. reduced to a fine powder in an agate mortar, and afterwards levigated, were mixed with a sufficient quantity of caustic potassa, and exposed to a red-heat in a silver crucible. By an hour's continued heat, I found the mass but half fused. It was then extracted from the crucible by water, and treated in the usual manner with muriatic acid, which left 0,238 gram. undissolved. This residue was again exposed to a red-heat with potassa, and dissolved in muriatic acid: the undissolved portion now weighed 0,137. After once more repeating

peating its ignition with potassa, the portion insoluble in muriatic acid was lessened to 0,108, which on trial was found to be pure silica* (*a*).

All the solutions, together with the water used for edulcorating the precipitates, were then precipitated with caustic ammonia in the least possible excess. The well-washed precipitate, after exposure to a red-heat, weighed 0,507 gram.; these were dissolved in sulphuric acid, leaving a residue of 0,007 gram. (*b*) of silica, and the solution gave a precipitate with caustic potassa, which was dissolved by adding more potassa, and only left a few unappreciable flakes of oxide of iron behind. That portion, therefore, which was dissolved in sulphuric acid was alumina, the quantity of which, after deducting the separated silica, amounts to 0,500 gram. (*c*).

For the sake of certainty, the solution in caustic potassa was saturated with muriatic acid, until the precipitate became redissolved; after which carbonate of ammonia was added to great excess, but no glucina or magnesia could be discovered; and the filtered fluid remained perfectly clear even when boiling, and after the excess of ammonia had been expelled.

The fluid, precipitated by caustic ammonia, was neutralized with muriatic acid, and mixed with a few drops of oxalate of ammonia; but after the lapse of twelve hours, not the least sign of turbidity appeared, and on boiling it with subcarbonate of potassa, no precipitate could be produced.

0,614 gramm. of this fossil have therefore yielded,

			In 100 Parts.
Silica (*a*)...	0,108		
(*b*)...	0,007...........	0,115	18,73
Alumina (*c*)		0,500	81,43
		0,615	100,16

18,73 parts of silica contain 9,42 of oxygen, and 81,43 parts of alumina contain 38,03; but $9{,}42 \times 4 = 37{,}63$; by this ratio the formula of chrysoberyl becomes $A^4 S$.

Boracite from Lüneburg.

Professor Stromeyer mentions in Gilbert's Annals, vol. xviii. p. 215, that he had found this mineral to be compounded of 67 boracic acid and 33 magnesia. As the analytical experiments of Professor Stromeyer have in general gained so just a confidence, one could not well doubt the correctness of the

* In order to convince myself that the silica I obtain in my experiments is pure, I am accustomed to dissolve it by fusion in a good quantity of subcarbonate of potassa. If the mass dissolve in water without any residue, I take it for granted, that the silica is not contaminated by any other earth.

the above statement: but since we are still in want of his account of the manner in which the analysis was performed, it is the more difficult to decide upon, because every method hitherto known of separating boracic acid from its combinations has but incompletely answered its object. From some experiments which I have made for the purpose of discovering the composition of boracic acid, by means of its capacity of saturation, I have found, that if a boracic salt, borax for instance, be mixed with from three to four times its weight of finely-powdered fluor-spar, free from silica, and a sufficient quantity of concentrated sulphuric acid, and the mixture be evaporated to dryness and exposed to a red-heat, that in this manner the entire proportion of boracic acid may be expelled, in the form of fluo-boracic acid. If, after this, the quantity of the base is determined, then the composition of the salt is at the same time obtained*.

This analytical method is naturally applicable to all boracic salts with fixed bases, which can be decomposed by sulphuric acid; and as the boracite belongs to this number, I have been enabled to repeat Professor Stromeyer's experiment, in hopes of obtaining a result in some degree worthy of reliance.

In order to free the boracite from any possible admixture of its matrix, consisting of sulphate of lime, a portion of the finely-powdered mineral was repeatedly washed and levigated with water, after which it was placed on a filter, washed and dried.

Of this powder 0,849 gram. were mixed in a platinum crucible with three grammes of finely-powdered Derbyshire fluor-spar, concentrated sulphuric acid being afterwards poured over it, with due precaution, in order to avoid the spirting up during the evolution of the gas; then desiccated, and finally ignited. For the sake of obtaining a certain result, the mass was once more treated with sulphuric acid, but the peculiar smell of the fluo-boracic acid could not be perceived this time; which proved that the decomposition had been completed in the first process.

The sulphate of magnesia was afterwards extracted with water, and the undissolved part washed on the filter, until I could be quite certain that nothing of the sulphate of magnesia remained amongst the sulphate of lime. The filtered

* I have found in two experiments made in this manner, that borax, deprived of its water of crystallization, consists of

1.		2.	
Boracic acid	68,6	Boracic acid	69,2
Soda	31,4	Soda	30,8
	100,0		100,0

and

and neutralized liquid was finally freed from the lime contained in it, as gypsum, by means of oxalate of ammonia, was reduced to dryness, and exposed to a red heat. The salt obtained in this manner weighed 0,758 gram., and proved on trial to be pure sulphate of lime. The quantity of magnesia contained in it amounted to 0,257 gram., and the deficiency in 0,849 gram., or 0,592, must consequently be boracic acid.

100 parts of boracite contain, according to this analysis,

Boracic acid	69,7
Magnesia	30,3
	100,0

Gay-Lussac and Thenard have found boracic acid to contain 33 per cent. of oxygen. If this be the case, then the quantity of oxygen in 69,7 parts of acid amounts to 23; 30,3 parts of magnesia, on the other hand, contain 11,73 parts of oxygen; but $11{,}73 \times 2 = 23{,}46$, that is to say, the boracic acid would contain twice as much oxygen as the magnesia. So long as the composition of boracic acid remains in dispute, I will not quote it as a proof of the correctness of my analysis, by which Gay-Lussac's and Thenard's statement may perhaps receive support.

LXXII. *On the Origin and Production of Matter, and on its alleged Infinite Divisibility.*

To the Editors of the Philosophical Magazine and Journal.

SHOULD the following remarks on the Origin and Production of Material Substance, and on the Infinite Divisibility which is usually, though, I believe, erroneously, ascribed to it, appear to be sufficiently philosophical, I shall be gratified by their insertion in your useful Journal.

I am, Gentlemen, yours respectfully,

J. O. F.

Bulk and extension pre-suppose solid elementary particles, of which forms are compounded; for if there be no such primary solid particles, there can be no material solidity whatever, either primary or derived; thus no material bulk or extension,---which is absurd.

A solid or primary particle of matter must be the smallest particle, and can admit of no divisibility; for if it can be divided into parts, it is not a solid or primary or the smallest particle; matter is therefore not infinitely divisible.

That original, elementary, or solid particle of matter, which admits

admits of no further division, must be the smallest particle of matter; and to say that there is no such thing as this smallest particle, is the same as to affirm that there are no material forms at all;---because it is to affirm that a whole can exist without the parts necessary to compose it.

From this it follows, that there is but one elementary principle in matter, of which principle the primary particle above mentioned consists;---and that all material subjects are forms compounded by motion arranging and co-arranging this elementary principle in innumerable relations and modes; for if there be primary material particles, these must be innumerable, in order to their entering into and producing the innumerable forms and combinations of forms observable in the material universe. It is, moreover, in accordance with reason and observation, thus to consider the original substantiality of matter, whence arises our idea of a simple or primary particle of material substance, called an atom;---a congregation of which atoms, by modes of motion, furnishes the idea of natural compounds, or material subjects as they exist in nature, in all their varieties: for as it is evident that modes of motion produce changes in material subjects, by transforming them into other material subjects of a totally different form and quality, so analogy points to the conclusion,—that all differences in material subjects, as they exist in nature, are effects of motion disposing primary particles into forms, and then operating successive and various combinations of those forms; and thus, that what is called Chemical Action, is, when considered in its origin, nothing more than an effect of motion in the more refined and subtle orders of substances;—decomposition being effected by opposing forces, composition by attractive forces; and thus also, Chemical Action, like that which is called Mechanical, is resolvable into an effect of motion.

If the above observations be founded in truth, the law of infinite series in numbers is totally inapplicable to the division of matter; and such erroneous application of that law must arise from confounding the distinct properties of numbers and of matter: this will appear evident, when we consider, that by the doctrine of numbers we can conceive that which is absolutely and originally one, and indivisible in itself, to be itself divided even infinitely;—which is absurd. Thus by means of numbers we can conceive the First Cause to be divided into two or three, or an infinite number of First Causes, thereby destroying its existence! This error arises, therefore, from conceiving that the properties of numbers are applicable to simples or elements, in the same manner as they are applicable to compounds; but as substance is prior to any

 thing

thing that can be conceived or predicated of it, and is what it is in itself, whatever is affirmed of such substance, therefore, can be no otherwise true than as it is found not to contradict the properties and modes, which are discovered to be true respecting it, and which are essential in its nature. Thus the doctrine of numbers is not applicable to particular substances, except in a manner limited and determined by the properties and modes of the existence of such substances themselves; and not in an arbitrary manner:—any other application of number is merely ideal and notional; for it is evident that in a substance, for instance, which is in itself a compound, the idea of number is applicable according to the several simples, unities, or elementary parts of which it is compounded; whereas the unities, or simples, which form it, in themselves, individually considered, admit of no application of number whatever; but that of unity only. *To divide a first principle is to destroy it!*

Number, therefore, is the natural measure of what is compounded; unity is the measure of what is simple: if it be not so, and if number be universally applicable to all substances, then there is no such thing as a simple substance, and by consequence no such thing as a First Cause, or Original Entity or Substance;—which is absurd. And this leads again to the conclusion, that all compounds whatever must be formed from simples or primaries. But as the primaries or simples which form material compounds, are not in themselves First Causes, that is, they are not self-subsisting, but created and derived,—it follows, that they must be *simple effects, resulting from causes of a superior order:*—we will endeavour, therefore, to enter into a somewhat further investigation of their nature.

If there be any substance in itself simple, and thus, *in its present or actual state of existence*, incapable of being divided, such substance will be, *in its present or actual state of existence*, incapable of being modified; for every modification of such substance, *while it remains in its present or actual state of existence*, presupposes re-arrangement; and there can be no re-arrangement without parts, which it has not: it follows then, if a simple particle of matter cannot, *in its present or actual state of existence*, be at all modified, because it is without parts,—that in order to its being modified it must be resolved into something of a totally different nature, and *not material;* and which may be called *immaterial;* for an indivisible material particle may thus be conceived of as creatable and created from an *immaterial* or spiritual origin, or substance;—as a natural simple effect, resulting from a spiritual cause.

To say that such simple and indivisible particle of matter cannot be modified at all, is to suppose it uncreatable in itself, thus

thus self-subsisting,—thus a First Cause, as having no antecedent whatever,—thus infinite;—and if infinite, as there can be but one infinite, there can be but one such particle of material substance, which is absurd; such a supposition would render creation impossible, by requiring *a number of infinite things to constitute its parts!* Such a doctrine, if for argument sake it were admitted, would place Deity in matter itself,—in the lowest sphere of created existence instead of in a sphere infinitely above all created existences, the life and subsistence of which are the effects of a continued momentary emanation from Him;—whereas nothing but a derived life and existence appear to belong to matter, and those of the lowest order. Nothing, for instance, of motion, except in a derived and secondary degree, appears to belong to matter, much less the power in which motion finally originates, namely Life itself, Life in its origin,—or the Divine Essence. Material substance then, considered in itself, or in its origin, is a passive substance, the simple effect or created result of *immaterial* substance. The visible and invisible worlds are no doubt thus mysteriously connected with each other, throughout the various gradations of Cause and Effect, from the Divine Artificer himself down to the lowest things of his creation:—and if these lowest things were for a moment disconnected and independent of their causes, they would lose their support, and instantly perish: so necessary is the constant exercise of Divine Power to uphold that which it has created, that subsistence may justly be termed perpetual creation. But how this connection is carried on, and in what manner *material* substance is formed or created from *immaterial* substance, can no otherwise be explained than by considering them in the relation of cause and effect; by the thing prior operating a medium whereby it may descend into the extreme or ultimate; this prior thing being itself a created existence from the only self-subsisting existence, which is God: the whole being thus the work of God in successive order.

The primary particles of matter, or the substances of which the material universe is compounded, appear evidently to be passive, and to be operated upon by active substances of a higher order in creation: and that this is the case, may be concluded from observing the various subjects in nature, to the life of which matter may be said to serve as a fixing or ultimate medium, or instrumental basis; for nothing of life appears to belong inherently to the material substances composing those forms or subjects in nature: on the contrary, the material substances composing such forms, seem to contain and to be operated upon by *interior* forms of life, actuat-

ing and disposing them, by modes of motion, into outward forms corresponding to such *interior* or inward forms.

Matter, then, considered in itself, is a passive substance, created as an instrument, or medium, for the development of an active, living, immaterial substance, in the ultimate or lowest degree of existence, namely, in nature; and this by being made the passive subject into which such living and active subject may enter and manifest itself. Would not such a doctrine, if fully developed, satisfactorily explain some of the first principles of the economy of nature, and prove the presence of the invisible in the visible world, and the order of life therein? Under this view, the natural universe is primarily divisible into two *universals* or principles which enter into every particular of which it is constituted; namely, the active, immaterial, or spiritual; and the passive, material, or natural; the latter being created from, and for the use of the former, and being the last result of the Divine Operation.

I now proceed to offer some further proof of the defective state of science respecting the solidity and divisibility of elementary matter, by examining the doctrine upon these subjects, as laid down in a standard work on natural philosophy not long since published; in doing which I beg to premise, what is indeed deducible from the views I have already advanced, that my arguments do not at all oppose themselves to the *facts* resulting from the *indefinite* divisibility of matter, as these facts are exhibited and brought forward by modern philosophers; but, on the contrary, are calculated to exemplify the true limit and nature of this divisibility, as it is illustrated by experiment, and to confine it within the bounds which appear to belong to it, by overstepping which, philosophers seem to have formed erroneous conceptions respecting these very facts, as regards their application to illustrate the first principles of Nature.

Professor Millington, in a recent work of great merit, his Epitome of Natural Philosophy, proceeding upon the received opinions entertained respecting original or elementary matter, enumerates five properties as belonging to "*original uncompounded* or *primitive*" particles of matter; the second of which properties is, that they are "*infinitely divisible*," and the third, that they are "*impenetrably hard*." Now it may be asked, if one of these particles be impenetrable, how can such particle be divided, since division, or a separation into parts, implies *perviousness* or *penetrability?* And may it not hence be concluded that a substance, the simple and component parts of which are *impenetrably hard*, must itself be *finitely divisible?* that is, its divisibility must be *limited*, although allowed to be *indefinite*,

indefinite, or beyond the reach of experimental or assignable limitation? Whence it follows, that if the primitive or uncompounded particles of matter are considered to be *infinitely divisible*, they must also be considered to be in like manner *infinitely pervious or penetrable*—thus *infinitely compressible*, which is contrary to reason and experiment; and from which it also follows, that matter, *not being infinitely penetrable*, cannot be *infinitely divisible*, but only *indefinitely* so.

The author then goes on to illustrate the doctrine of infinite divisibility, by the separation and attenuation of certain substances; in doing which, however, he proves nothing further than that those substances are divisible *indefinitely*, or in a greater degree than our means will enable us to effect their separation, as in the instance given by him of pulverized marble; and in a greater degree than our senses, aided by instruments, can enable us to perceive, as deducible from the instance cited of the animalcula counted by Mr. Leuwenhoek; which facts merely go to prove *how indefinitely*, and not *how infinitely*, the division of matter may take place.

" The second property of matter," says the Professor, " is that it is infinitely divisible, or in other words, that the original component parts, or elementary particles of which all things are formed, are small beyond conception: thus, if marble or any brittle substance is reduced to the most impalpable powder which human art can produce, its original particles will not be bruised or affected; since if this powder be examined by a microscope, each grain will be found a solid stone, similar in appearance to the block from whence it was broken; and of course, if we possessed suitable implements, would admit of being again subdivided or reduced to a still finer powder. If a single grain of copper is dissolved in about fifty drops of nitric acid, and the solution is afterwards diluted with about an ounce of water, it is evident that a single drop of it must contain an almost immeasurably small portion of copper, and yet so soon as this comes in contact with a piece of polished steel or iron, that metal will become covered with a perfect coat of copper; consequently as much iron may be covered with copper as the solution will wet; which shows how infinitely the copper can be divided without any alteration in its texture.

" Gold is so extended under the hammer of the workman, in forming it into the thin sheets called leaf-gold, that the 500,000th part of a grain becomes visible to the naked eye, or the 50,000,000th part, through a microscope magnifying but ten times: and Mr. Ferguson has calculated that a single pound of gold would be sufficient to cover a silver wire capable

pable of encompassing the earth, or about 24,000 miles in length! But the wonders of art sink into nothing, when compared with those which nature produces; for Mr. Leuwenhoek, the celebrated microscopic observer, affirms, that he has counted two millions of animalcula in a portion of the milt of a cod-fish not longer than a common grain of sand!! That matter is thus infinitely divisible, also admits of demonstration on mathematical principles."

In the above cited passage, the infinite divisibility of matter is defined by its being said, that the elementary particles of which matter is composed, are "small beyond conception," by which term I apprehend, nothing more can be understood, than that an elementary particle of matter is *a body of insensible magnitude;* for *magnitude* is certainly affirmed of it, although not of a nature sufficiently gross to be obvious to the senses. Let this magnitude then be the *smallest possible*, and it will be a magnitude that, as we have already seen, *cannot be divided:* whence it appears that a thing "small beyond conception," or rather beyond the powers we possess of bringing its magnitude into sensible perception, is not on that account *infinitely divisible*, but only *indefinitely* so. In like manner, in the instance which is adduced of the attenuation of particles of copper in solution, the author observes, that a single drop of the solution, containing but an "almost immeasurably small portion of copper," will deposit, by the division of its particles, a coat of copper upon as large a surface of steel as the drop will wet; which," he proceeds to observe, "shows *how infinitely* the copper can be divided." But such a conclusion by no means follows: for the term "*almost immeasurably small*" implies, at most, nothing further than the state of *indefinite division;* and this state bears no relation to that (ideal state) of *infinite division.* The same may be said of the divisibility and tenuity of gold; namely, that those qualities afford no proof that gold is infinitely tenuous or divisible; for even although a pound of that metal, as instanced by the Professor, may be sufficiently tenuous to be made to cover a silver wire capable of encompassing the earth, yet this, so far from proving it to be infinitely divisible, does not even prove that it would gild a wire capable of encompassing the planet Jupiter: and there are yet many far greater and readily conceivable finite magnitudes,—the orbits of the planets, for example.

With respect to the assertion, "That matter is thus infinitely divisible, also admits of demonstration on mathematical principles"—this, as I have before shown, cannot be maintained, unless it can be proved that the *point, origin, or first principle,*

from

from which geometrical quantity or solidity commences, will admit of something more than an imaginary division; for until this is done it cannot be assumed to be divisible, and therefore mathematical science must be inapplicable for the purpose of dividing it. If indeed we first *assume a primitive particle of matter to be a compounded material substance,*—which, as I take it, would be to *assume a simple to be compound,*—mathematics may then be applied to divide it *ad infinitum:* but if, after all, there be any real limitation in such (supposed) compound, the application of mathematics to divide it, *beyond such limitation,* must necessarily be a fiction.

The term *infinite* is, I think, philosophically inapplicable* to any and every created thing, and *is applicable only to the attributes of the* CREATOR!

In concluding these observations on the origin and alleged infinite divisibility of matter, I beg it may be fully understood, that I have selected for criticism the work of Professor Millington from among the various works in which the doctrine of the infinite divisibility of matter is asserted or maintained, which are all, as I conceive, equally in error upon the point in question, solely because it conveys, in my opinion, a just view of the present state of science upon the subjects of natural philosophy of which it treats; and it should be particularly adverted to, with respect to this highly useful volume, that my objections apply to first principles only, and do not affect what may be called the tangible and experimental properties of matter, as explained in it; and which properties are applied by the author to elucidate the practical and experimental subjects he describes. It is only in that part which treats of the primary laws of matter, as the sources to which those experimental properties are attempted to be traced, that any error is attempted to be pointed out in the statements or conclusions of the author, upon whose valuable labours the writer of these remarks would be most unwilling to cast a shade.

LXXIII. *On the Petrifactions of Osterweddigen, near Magdeburg. By Professor* GERMAR. *Read before the Natural History Society of Halle, Feb.* 1, 1823.†

AT Osterweddigen, a German mile and a half from Magdeburg, is a stratum of sand, which is distinguished by its richness in fossil bivalve and univalve shells; but which, with

* What then shall we say of infinite duration, infinite space, and infinite series in mathematics? Is not the term infinite applicable to physical, as well as to moral and intellectual properties?—EDIT.

† From Schweigger and Meinecke's *Neues Journal,* band vii. p. 176.

with respect to the formation it belongs to, as well as to the genera and species of shells that it contains, appears to differ from the sand- and marl-strata of England and France, which likewise contain fossil remains.

This stratum consists partly of coarse, and partly of fine loose quartz-sand, somewhat of a greenish colour (on the surface at least), and varying in thickness from a few inches to more than a foot. It rests upon the new red sandstone (*? bunten sandsteingebürge*), which crops out there, the sand entering into its fissures and cracks; and is covered by the clay-marl (*mergelleimen*), which, as is well known, lies upon our brown-coal bed. It is difficult to decide whether this stratum of sand appertains to the brown-coal rock, or to the later clay-rock (*leimengebirge*), or whether it belongs to those formations which occur near Paris, as intervening between our brown-coal and clay-rock. A nest of brown-coal was indeed found beneath the clay (*leimen*), but the stratum of sand was so narrow at the place, that their relative situations to each other could not be ascertained.

In this sand, a great number of bivalve and univalve shells are found, partly as fossils, partly in the state of casts; and only a few of them retaining their nacreous lustre. The casts, on the contrary, consist of a dark greenish-gray, and mostly fine foliated, argillo-calcareous ironstone, and their surface is not unfrequently still coated with a thin layer of enamel. These casts are likewise contained in the lowermost layers of clay. In the sand, there are also found nodules of the same calcareous ironstone, which appear in places as united together by means of casts of the various genera, interwoven with each other; and these nodules are seldom found destitute of petrifactions. The occurrence of these fossil bodies, together with their proper casts, at the same time, becomes particularly interesting; because it enables us to compare the one with the other, and affords proof, that casts of shells assume, very frequently, a totally different form from that of the originals. It is also remarkable, that the secretion, as it were, of the solid argillo-calcareous ironstone from the mass of sand, appears to have been effected in a particular manner by the organic bodies, even if we could not ascribe to them any further influence than the affording, by their hollow spaces, opportunity and room for secretion; and that, where numbers were lying together, they presented collecting points for the mass.

The following is an enumeration of the fossil remains found here, which altogether originate from marine animals; and which, on that account, allow us to presume that they do not belong to the brown-coal formation; for that includes the remains of land- or fresh-water-animals only. No

No traces were found of chambered shells, except a couple of remnants of the so-called jointed *Dentalia*, one of them with the apex broken off, in the state of a cast, the other as a very acute cone, with its shell partly preserved; these shells were in all probability related to *Dentalium*.

A true *Bulla* Lam., the casts of which are known by the name of Physalites, was frequently met with in that state, but only twice as a fossil shell. It is of the size of a coffee-bean, is nearly cylindrical, its apex umbilicated, and has regular fine transverse striæ.

Of *Turbo* two small species were found, it seems, with an umbilicus: the one, almost perfectly conical, and striated longitudinally; the other, with a shorter spire, smooth, and appertaining perhaps to *Delphinula* Lam. They appear, however, to occur but rarely, and but few casts of them were found.

The genus *Turritella* likewise appears to have been rare at this spot. Two specimens, which were not complete, might indeed belong to two different species, and are of about four lines in length, but it is impossible to determine them more exactly.

Trochi were more abundant, but were found as casts only; with some fossil opercula, which perhaps belong to this genus, but which had joined concentrically, and not in a spiral form.

Of the genus *Natica* many casts were found; and several fossil specimens collected, which may belong to different species. One particularly distinguished species, of the size of a hazel-nut, has only from four to five volutions, a very flat and scarcely protruding spire, is closely and very finely striated in a spiral direction, and marked at greater intervals with undulated longitudinal striæ. Another and very similar species is rather smaller, nearly smooth, its spire more produced, and appearing to be plicated on the apex. Another, probably a species of this genus, has five or six scarcely protruded volutions, the largest of which, where it meets the other, appears pressed and driven in.

Of the genera *Conus* and *Cypræa* no remains were found, but some of *Voluta* and *Oliva* occurred; and of these, a species which possesses some resemblance to the smaller specimens of *Vol. glabella*, but which is unknown. Also a small species of the genus *Columbella*.

Whether real *Buccinites* were present cannot be exactly ascertained. Casts are abundant, which, according to the size of the first volution, might be placed amongst *Buccinites*; but they appear to have originated rather from *Voluta* and other genera than from *Buccinum*.

 Two

Two specimens were collected of a small *Cerithium*, quite smooth, and half an inch in length; and amongst the casts were some which appeared to belong to this genus.

Casts of the genus *Fasciolaria* were found in great abundance, all of which, however, appeared to belong to one or two species also found in a fossil state, and which, in the state of casts, would be ranged with *Buccinites*. Some casts likewise occurred, which were most probably derived from a *Pyrula*. Turbinated shells in the state of casts were not unfrequently found, which originated in all probability from species of the genus *Fusus*, and which presented three fossil shells of smaller species, one of them with its volutions from left to right.

Amongst Bivalves, the *Ostracites* occupied the first place with respect to their abundance. Single valves of a very thick-shelled oyster occurred frequently, the diameter of which sometimes measured about five inches, and which may perhaps belong to *Ostrea biauriculata* Lam. Smaller species also occurred, in part regularly grooved; but no specimens were met with of the *mantle pectens* or of *Cristacites*. The very strength of this shell (*Ostrea*) appears to have effected its preservation, for scarcely any casts of it were found; casts of bivalves were rare in proportion.

A small, elliptical, finely-ribbed *Terebratula*, with a perforated beak, and internal cartilage still preserved, perhaps belonging to *Terebratula radiata* Lam., was found in some specimens. A small concentrically striated shell was more frequently met with, the beak of which was not perforated, and which did not exhibit its cartilage, but was provided with a notch in the beak, below the hinge, through which the muscle of attachment had probably passed. This species appears to belong to a peculiar genus hitherto unknown.

The genus *Arca* yielded two species, one of the breadth of half an inch, finely decussated, the margin not crenulated, and the hinge very narrow; and one, a fourth of the size, with distinct excentric ribs and toothed margin. Two or three small species of *Pectunculus* occurred, but not in specimens sufficiently distinguishable.

Cockles were rare: some imperfect specimens were found, however, which undoubtedly belonged to the genus *Cardium*. Some casts appeared, by their outlines, to belong to *Tellina*.

Of *Veneres*, occurring so frequently amongst fossil shells at other places, two species only were found here, and those not rare ones; the one a larger, of about four lines diameter, with fine distant concentric stræ, and another of half the size mare distinctly and more closely striated. Two species of the genus *Venericardia* Lam. were found pretty frequently, both

both strongly ribbed longitudinally, and toothed at the margin; the larger one was of about nine, the smaller of about three lines in length. On some casts the impressions of the abductor muscle were very strongly marked.

Many of the Bivalves and Univalves were frequently found with round holes bored into them, which indicated the presence of some predaceous *Trachelipodes* (*? Bohrmuscheln*), although none of them were found. We met with, however, a cylindrical irregularly bent tube, which might have originated from a *Teredo*, but perhaps from a *Serpula*.

Dentalites were dispersed about in great numbers, yet always as casts only, very rarely in single fossil fragments. They were an inch in length, two lines wide at the base, their transverse section circular, and tapered uniformly and with a gradual flexure towards the apex. The shell appeared to have been smooth.

Of *Corallines* single bits of *Madrèpores* and *Millepores* were observed. But a species of Coral likewise occurred, consisting entirely of cylindrical branches, variously grouped together, without a mutual trunk; and the hollow spaces of which were every where filled with sand, which prevented an examination of the surface of these branches. They formed, as it appeared, small running banks in the sand.

Whether *Echini* also existed here cannot be ascertained with certainty; but certain bodies appeared, which most probably were fragments of their spines, though no further traces of them were discoverable.

Teeth of fishes, or the so-called *Glossopetra*, might be collected in abundance; and, if we may be permitted to conclude, from the variety of their form, upon the variety of animals to which they belonged, they indicated several species of predaceous fishes not of great size.

The above are the genera and species of fossil bodies I have observed in this place; manifestly real marine productions, and, with the exception of oyster shells, of proportionably small size. To judge from the frequency of the individual shells met with, I should characterize this stratum of sand by the genera *Bulla*, *Natica*, *Fasciolaria*, *Ostrea*, *Venus*, and *Venericardia*. My endeavours to determine the species more exactly after Lamarck were in vain, and I am compelled to conclude, that amongst the fossil shells of France there are few or no species identical with those found near Magdeburg.

Cuvier, in the new edition of his Geological Description of Paris, enumerates the following series of formations, commencing with the chalk:—

 1. *Chalk*;

1. *Chalk*, with marine exuviæ.
2. *First Fresh-water Formation*, consisting chiefly of plastic clay, brown-coal, and sand. This is probably the same with our brown-coal formation, and contains fresh-water shells principally; but in the upper stratum both fresh-water and marine shells together; of the latter, *Cerithia*, *Ampullariæ* and Oysters, in particular.
3. *First Marine Formation.* Limestone and sand. The characterizing shells here belong to the genera *Cerithium*, *Lucina*, *Cardita*, *Cardium*, *Voluta*, *Ovulites*, *Turritella*, *Cytherea*, *Crassatella*, and *Corbula*.
4. *Second Fresh-water Formation*, containing siliceous limestone, gypsum and marle. The gypsum contains the well-known bones of remarkable land animals; but the marle lying above it always contains the remains of marine animals, particularly *Cerithia*, *Cytherea* and Oysters.
5. *Second Marine Formation;* compounded of gypseous-marle, sand, sandstone, limestone and calcareous-marle. Here the genera *Oliva*, *Fusus*, *Cerithium*, *Melania*, *Crassatella*, *Pectunculus*, *Cytherea* and *Ostrea* are particularly found.
6. *Third Fresh-water Formation*; consisting of marle and sand.

Our stratum of sand near Magdeburg must be referred, in all probability, to the second marine formation.

LXXIV. *Remarks on some of the American Animals of the Genus* Felis, *particularly on the Jaguar*, Felis Onca *Linn. By* T. S. Traill, *M.D. F.R.S.E. &c.* *

AMONG the genera into which Linnæus has distributed the higher animals, none seems more natural, or better defined, than the genus *Felis;* yet such are the vague descriptions given by most travellers, and by the older naturalists, that we are still in uncertainty respecting several of the species which compose it. My attention has been particularly drawn to this genus, by accidentally meeting with skins, and occasionally with living animals belonging to it, which I have in vain endeavoured to reconcile to the descriptions of authors; and the magnificent collection of zoological drawings in the possession of Lord Stanley has made me acquainted with several of the feline genus, which do not appear to have attracted the attention of our best systematic writers.

The feline animals belonging to the American Continent are numerous, and have generally been ill described by naturalists. Indeed there appears to be a singular prejudice respecting them in the minds of many zoologists. Because neither the

* From the Memoirs of the Wernerian Society, vol. iv. Part II. p. 468.

lion

lion nor the tiger (the monarchs of the forest in the Old World) is found in America, it was a favourite dogma with a celebrated author, that the beasts of prey of the New Continent were inferior in courage and ferocity to those animals of the Old World, which they most nearly resembled. It is true, that none of the beasts of prey of America equal in size and power the lion of Africa, or the great tiger of Bengal: but the jaguar, the puma, and black tiger of South America, equal in courage and ferocity the panther, leopard, and onca, the animals of the other continents which they approach most nearly in size and habit.

Buffon and some other writers have described the jaguar and puma as destructive to other quadrupeds, but as cowardly and fleeing from the approach of man. It is now well ascertained that Buffon has confounded the true jaguar of South America with the ocelot, a much smaller and less formidable animal; and his account of the puma seems to be taken from the descriptions of those who have only seen the animal in the vicinity of human civilization. That eloquent writer has admitted the commanding influence of the experience of human prowess in subduing the courage of even his favourite animal the lion. "A single lion of the desert will frequently attack a whole caravan; and if, after a violent and obstinate encounter, he experiences fatigue, instead of flying, he retreats fighting with a bold front to his pursuers. Those lions, on the contrary, who dwell in the neighbourhood of the towns and villages of India and Barbary, being acquainted with man, and having felt the power of his weapons, have lost their native courage to such a degree, that they fly from the threatenings of his voice, and dare not assail him. They content themselves with preying on small cattle; and will fly before women and children, who make them indignantly quit their prey, by striking them with clubs."

Had Buffon not been trammelled by a favourite hypothesis respecting the alleged inferiority of the animal kingdom in America, he would have seen that the writers who notice the cowardice of the larger beasts of prey of that continent, only speak of them as observed near European colonies, where their native ferocity has been compelled to acknowledge the superiority of human intellect and arms. Recent observations have shown how ill founded these speculations of the French naturalist have been.

Humboldt mentions many instances of the ferocious courage of the great jaguar. Among others, an animal of this species had seized a horse belonging to a farm in the province of Cumana,

mana, and dragged it to a considerable distance. "The groans of the dying horse," says Humboldt, "awoke the slaves of the farm, who went out armed with lances and cutlasses. The animal continued on its prey, awaited their approach with firmness, and fell only after a long and obstinate resistance. This fact, and a great many others, verified on the spot, prove that the great jaguar of Terra Firma, like the jaguaret of Paraguay, and the real tiger of Asia, does not flee from man, when it is dared to close combat, and when it is not alarmed by the great number of its assailants. Naturalists are now agreed, that Buffon was entirely mistaken with respect to the largest of the feline genus of America. What that celebrated writer says of the cowardly *tigers* of the New Continent relates to the small ocelots; and we shall shortly see, that on the Orinoko the real jaguar of America sometimes leaps into the water to attack the Indians in their canoes."

I am personally acquainted with gentlemen who have hunted the jaguaret in Paraguay, and who describe it as a very courageous and powerful animal, of great activity, and highly dangerous when at bay. Both this species and the puma are rendered more formidable by the facility with which they can ascend trees. I have been assured by several friends, who have repeatedly hunted the tiger in India, that even this "most beautiful and cruel of beasts of prey," as it is termed by Linnæus, generally endeavours to escape from the hunters, unless hard pressed, or surprised in a situation from which retreat is difficult; and one gentleman informed me, that, on a shooting excursion, to his great horror he found himself without a companion in a small field, in which he espied a tiger watching him; that, finding retreat impossible, he advanced against the animal firmly, when it slowly retired, until he had an opportunity of dispatching it with his rifle.

Such instances show that there is no striking difference between the habits and courage of the beasts of prey of the Old and New Continents, as imagined by Buffon.

While naturalists have been so unjust to the *character* of the American animals of this genus, the forms of these quadrupeds have not been more fortunately delineated in our engravings. For instance; the figure of the black tiger in Buffon, and in his copyist Shaw, is so wretchedly drawn, and its limbs are so distorted, that not a trace of the genuine form is preserved; but it is considerably better given in the respectable work of Pennant. The figures of the jaguar and puma, in both the former works, are inaccurate in many respects, especially in the form of the heads, and in giving no idea of the

fierce

fierce expression of the countenances. The figure of the ocelot, in Shaw, is an absolute caricature, and conveys no idea of the sprightly motions and strength of this beautiful miniature of the leopard.

These circumstances have induced me to lay before the society a fine drawing of a very beautiful jaguar from Paraguay*; which was some time ago alive in Liverpool. When the animal arrived, it was in full health, and, though not fully grown, was of very formidable size and strength. The captain who brought it could venture to play with it, as it lay in one of the boats on deck, to which it was chained; but it had been familiarised to him from the time it was the size of a small dog. I did not venture to take measurements of it; but it appeared to be between six and seven feet in length (including the tail), and to stand between two and three feet in height at the shoulder. The size of the fore-legs seemed very great in proportion to the bulk of the body, and especially of the hind-legs and rump of the animal. The ground-colour is bright fulvous; the fur is short, thick, and glossy, all over the body. It is variegated by long chain-like spots. A chain of such spots passes down the spine from the shoulders to the tail, which consists chiefly of single spots; but some of them are double. On each side of this chain are several rows of open spots, formed by a glossy border of black, including one or more spots of the same colour. As they descend the sides of the animal, these borders become interrupted, and present the appearance of clusters of four irregular oblong spots, with occasionally one or more small central dots. Viewed from above, the back has no inconsiderable resemblance to the markings of the shells of some species of tortoise, from the peculiar arrangement of the colours, and the equality of the spaces between each cluster of spots. The face, sides of the neck, and both sides of the legs, are thickly studded with small black spots. The ground-colour of the lower part of the body and inside of the thighs is dull-yellowish white; but the belly is spotted with large, black, irregular marks.

The hair of the tail is not glossy: its upper part is marked with a zigzag pattern, as in the figure; and its lower part is annulated with two or three broad blackish-brown rings, separated by dull yellow stripes. There are two distinct sets of vibrissæ; the first of which are the longest, and are placed two or three inches before the scanty hairs of the other set. The teeth are very large and strong. The whole animal had

* The drawing was made by Mr. Alexander Mosses, a young artist of great merit, who was employed by me for this purpose, and has succeeded admirably in giving the character of the animal.

an

an appearance of activity and strength, which fully confirmed the accounts of its prowess collected by Humboldt.

Felis Puma.

For this animal I would propose the following specific character, which appears necessary to distinguish it completely from *Felis unicolor*, described by me in the third volume of the Society's Memoirs.

Felis, *corpore dilutè badio; auribus nigris; caudâ claviformi, apice nigricanti.*

Cat, *with a light-bay body; black ears; a claviform tail, brownish-black at the tip.*

I had an opportunity of inspecting several skins of this animal, the property of Mr. Edmonston, who had killed them in the interior of Demerary. None of them were without the marks indicated in the specific character. The whiskers of all arose from a dark-coloured spot on the face. The blackish tip of the tail measured five inches; and, from the length and position of the hairs, made the extremity the thickest part of the tail, or gave it a claviform shape. One of these animals was a female, shot while searching for prey in a lofty tree: its whelp was at the bottom, feeding on a monkey, which had probably been killed by the mother. The young one was also shot. The body of the latter measured, from nose to tail, two feet, and the tail one foot one inch. The upper part of the body was not of an uniform colour like the dam, but it had three chains of blackish-brown spots along its back, with several scattered markings of the same colour on its sides, neck, and shoulders. The crown of the head had several obscure stripes; but the blackish spot at the roots of the vibrissæ, and the black backs of the ears, were very conspicuous. The lower part of the body, and the insides of the limbs, were of a dirty yellowish-grey, with dull brown bars. These marks disappear in the full-grown animal.

The largest of Mr. Edmonston's specimens seemed an animal of prodigious power. It had a much larger head, in proportion to its size, than the figures of Buffon and Shaw; and its canine teeth were enormously large. The dimensions are as follow:

	Feet.	Inch.
Length from nose to tail	4	9
——— of tail	2	6
Total length	7	3
Length of the head	1	0
Circumference of ditto	1	$9\frac{1}{2}$
Length of the large canine teeth above the jaw...	0	$1\frac{1}{4}$

Liverpool, November 1822.

LXXV. *On*

LXXV. *On the Adjustment of the Line of Collimation of the Transit Instrument.*

Gentlemen, Bath, November 10, 1823.

I HAVE recently met with a description of a mode of adjusting the line of collimation of the transit instrument, which, although published *upwards of thirty years ago*, does not seem to have been generally (if at all) practised; but which appears capable of great accuracy. It is to be found in a work entitled *Fixarum præcipuarum Catalogus Novus*, by F. de Zach, published at Gotha in 1792; in page 18 of which he directs the observer to note the exact time of the transit of a star (near the pole) from the *first* to the middle wire of the telescope (M. Zach's telescope having only *three* wires): which being done, he is then to invert the telescope, end for end, and note the exact time of its passing the *last* wire, which is obviously the very *same* wire as that which was called the *first* in the former position. If the two intervals correspond, the line of collimation may be considered accurate: but, if not, the proper corrections must be made to bring them so. As this method is very simple, and must be well known to many persons, I am surprised it has not been more generally adopted.

The same author, who has, I am informed, written other works on Astronomy, adds that a similar method may be successfully pursued with stars *not* near the pole. In this case, two stars must be selected which differ but little from each other in declination, and which can be observed without moving the telescope. As to their difference in right ascension, all that is required will be sufficient time for inverting the telescope. Let this difference in right ascension be well ascertained, either by actual observation, or deduced from the best catalogue. Let the transit of the first star be observed: then, after inverting the telescope, let the transit of the second star be observed. If the interval between the two transits made in this manner corresponds with the difference between the *correct* right ascensions of the two stars, the line of collimation is, in this case also, accurate: if not, it must be corrected as before mentioned. I am not aware of any objection to either of these methods: but, as I have not had much experience in practical astronomy, I submit them to such of your readers as may be curious in these matters: in order that, by a more general circulation, the method may have a fairer chance of being tried; and, if found successful, of being universally adopted.

I am, gentlemen, your very obedient servant,

Zeno.

LXXVI. *List of Occultations for the Year* 1824, *computed for the Meridian and Parallel of* Greenwich. *By M.* Inghirami *of Florence.*

[Concluded from p. 279.]

Day.	Star.	Mag.	Cat.	Æ	D	Im.	Em.	Dist. Im.	Dist. Em.
				SEPTEMBER.					
				° ′	° ′	h m	h m	′	′
3	Sagitt.	7·8	L 13	291 57	21 46 S	8 20	9 12	7 S	13 S
—	———	8	L 13	292 1	21 43	8 25	9 27	4 S	12 S
4	ρ Capric.	5	P 142	304 22	18 28	8 8	9 18	3 S	12 S
—	———	7·8	P 144	304 24	18 31	8 19	9 11	7 S	15 S
—	———	6·7	P 145	304 24	18 5	8 56	9 20	15 N	10 N
5	———	7	L 8	316 25	14 9	9 42	11 0	9 N	4 S
—	———	7	L 8	316 7	14 4	9 44	cont.		
—	———	7	L 8	317 8	13 52	11 43	12 56	4 N	9 S
6	Aquarii	7·8	L 10	326 37	10 33	6 34	7 13	7 S	14 S
—	———	7·8	L 10	327 58	9 30	10 36	11 49	12 N	3 S
8	9 Piscium	6	P 84	349 15	0 2 N	8 30	9 23	5 N	3 N
—	———	9	P 119	351 2	0 54	13 31	14 45	5 N	11 S
—	16 ———	6	P 132	351 33	1 0	15 13	15 22	11 S	13 S
11	104 ———	6·7	P 136	22 8	13 16	6 20	7 10	4 S	16 S
12	Arietis	6·7	P 112	35 37	17 59	8 54	9 32	15 N	8 N
13	Tauri	6·7	L 8	51 36	21 58	15 28	16 4	15 N	12 N
14	———	7	L 13	64 27	23 6	12 40	13 23	9 S	13 S
15	———	6	P 135	80 49	23 54	15 36	16 37	5 S	6 S
—	———	8	Z 332	80 43	24 9	15 49	16 41	10 S	9 S
17	61 Gemin.	7·8	P 98	108 47	20 39	12 30	13 20	5 S	2 S
29	Sagitt.	9	P 81	274 33	24 11 S	7 9	8 16	8 N	2 N
—	24 ———	7	P 99	275 15	24 14	8 46	9 39	5 S	11 N
—	———	6·7	P 105	275 25	24 10	9 8	10 6	3 S	10 S
—	———	8	P 103	275 24	24 15	9 12	9 52	9 S	14 S
—	———	6·7	L 13	275 25	24 10	9 29	10 21	5 S	11 S
30	———	7·8	L 13	287 4	22 10	6 14	6 35	15 N	13 N
—	———	7	L 13	287 25	22 27	6 34	7 36	5 S	12 S
—	———	5	L 13	287 52	22 8	7 46	8 59	6 N	4 S
—	———	8	L 13	288 23	21 56	9 6	10 16	7 N	2 S
—	50 ———	6·7	P 103	288 36	22 9	9 21	9 59	9 S	14 S
—	———	8	P 104	288 39	21 38	10 6	cont.		
				OCTOBER.					
				° ′	° ′	h m	h m	′	′
2	Capric.	7	L 8	313 52	14 45 S	12 7	13 0	3 N	7 S
3	Aquarii	7·8	L 10	325 19	10 24	12 27	13 32	4 N	10 S
—	———	7·8	L 13	325 19	10 18	12 31	13 37	8 N	5 S
—	———	7·8	L 13	326 38	9 34	15 44	16 31	12 N	2 N
4	63 ———	6	P 166	336 51	5 15	13 44	14 38	13 N	1 N
5	8 Piscium	5·6	P 83	349 10	0 10 N	16 52	17 47	6 N	6 S
8	101 ———	6	P 118	21 16	13 38	12 18	cont.		
9	Arietis	6·7	P 112	35 37	17 59	16 18	17 26	3 S	4 S
10	98 ———	7	P 261	44 28	19 59	7 6	7 31	15 S	11 S
—	58 ———	5	P 11	45 51	20 18	9 26	10 22	9 N	2 N
11	Tauri	7·8	L 11	59 6	22 32	9 2	9 40	13 N	8 N
—	———	7	L 13	61 52	23 5	14 46	15 40	11 N	7 N
13	1 Gemin.	5	P 307	87 59	23 16	8 32	9 15	3 S	7 S
—	12 ———	8	P 53	91 48	23 20	15 31	16 10	12 N	13 N
14	———	7	L 9	104 25	21 36	12 35	cont.		
—	56 ———	5·6	P 69	107 32	20 48	18 16	19 24	3 S	7 N
15	Cancri	8	P 295	118 28	18 11	11 25	12 15	4 N	2 N

Day.	Star.	Mag.	Cat.	Æ	D	Im.	Em.	Dist. Im.	Dist. Em.
				° ′	° ′	h m	h m	′	′
17	16 Sextant.	6	P 253	149 41	7 9	18 18	18 29	16 N	13 N
19	Virginis	6	P 167	175 12	4 13 S	15 17	15 52	15 S	6 S
20	——	5·6	L 10	190 51	10 30	18 50	19 39	2 N	14 N
26	Sagitt.	7	L 13	268 1	24 24	4 51	5 27	15 N	12 N
—	——	7	P 342	268 14	24 24	4 56	5 35	14 N	12 N
27	——	6·7	P 255	282 23	22 58	5 34	6 43	2 N	5 S
28	——	8	L 13	298 48	19 49	7 49	8 0	14 N	12 N
29	Capric.	6·7	P 240	307 19	16 49	4 45	5 47	12 N	3 N
30	Aquarii	7·8	P 119	318 53	12 56	4 18	5 32	7 N	5 S
—	——	9	P 130	319 18	12 47	5 25	6 42	3 N	1 N
—	——	10	P 131	319 19	12 58	5 29	6 21	5 S	14 S
—	——	7·8	P 126	319 12	12 31	6 4	cont.		

NOVEMBER.

Day.	Star.	Mag.	Cat.	Æ	D	Im.	Em.	Dist. Im.	Dist. Em.
				° ′	° ′	h m	h m	′	′
2	19 Piscium	6	P 182	354 3	2 23 S	10 19	11 34	7 N	8 S
3	45 ——	6	P 65	3 51	6 35	7 5	8 6	13 N	1 N
4	——	7·8	L 11	16 53	12 1 N	10 44	11 46	14 N	2 N
5	(*Query?*)	7	L 8	39 43	18 17	5 23	cont.		
—	Arietis	7	L 10	29 28	16 14	11 16	12 27	10 N	1 S
—	——	7	P 261	44 28	19 59	15 27	16 23	4 S	9 S
—	58 ——	5	P 11	45 51	20 18	18 12	19 2	3 N	1 S
7	Tauri	7	P 166	54 33	21 37	7 6	8 1	0	5 S
—	32 ——	6	P 197	56 16	21 53	10 38	11 23	8 S	13 S
8	——	7·8	L 11	70 16	23 11	10 0	10 52	6 S	10 S
—	——	8	Z 279	71 15	23 37	11 51	12 41	11 N	9 N
—	——	7	P 243	71 25	23 37	12 12	13 5	10 N	7 N
9	Orionis	8	P 192	82 48	23 6	6 15	6 56	4 S	5 S
—	Tauri	7	L 9	84 46	23 19	9 35	10 27	4 N	1 N
—	Gemin.	5	P 307	87 59	23 16	15 54	16 54	3 N	7 N
—	3 ——	6	P 340	89 24	23 8	18 49	19 30	8 N	12 N
—	4 ——	7	P 344	89 35	23 1	19 8	19 58	3 N	8 N
10	——	7	L 9	98 48	21 54	8 23	9 12	1 N	2 N
—	36 ——	6·7	P 247	99 53	21 59	9 58	10 42	9 N	10 N
11	81 ——	6	P 194	113 38	18 59	8 51	9 38	5 S	1 S
—	——	7	L 9	114 3	18 51	9 49	10 21	11 S	8 S
—	——	6·7	L 13	114 5	18 51	9 52	10 24	11 S	8 S
—	——	7	L 13	114 11	18 42	10 25	cont.		
12	Cancri	7	P 225	131 58	18 54	17 25	18 42	3 S	10 N
13	Sextant.	7	L 10	142 31	9 46	13 4	13 53	4 N	14 N
—	——	6·7	L 10	144 2	9 32	14 3	15 6	3 S	10 N
—	——	7	L 10	144 57	9 6	16 7	17 6	4 S	11 N
15	Leonis	7·8	L 10	169 37	1 50 S	12 58	13 45	12 N	0
—	87 ——	4·5	P 89	170 1	1 54	13 20	14 10	8 N	6 S
16	Virginis	8	Z 847	185 16	8 21	17 36	18 13	6 S	15 S
19	Libræ	7·8	L 10	227 38	22 8	19 5	19 45	15 S	8 S
24	Sagitt.	7·8	L 13	290 48	21 13	7 1	7 32	10 S	14 S
27	Aquarii	7·8	L 13	326 38	9 34	6 17	7 12	5 N	15 S
28	63 ——	6	P 166	336 51	5 15	3 37	4 45	14 N	2 S
—	——	7·8	P 183	337 37	4 38	6 51	cont.		
—	——	6·7	P 191	337 56	4 31	7 18	8 36	14 N	1 N
—	——	7	L 13	339 5	3 48	10 54	11 39	14 N	5 N
29	8 Piscium	5·6	P 83	349 10	0 10 N	7 44	8 23	7 N	14 N
—	9 ——	6	P 84	349 15	0 2	8 15	cont.		

Day.	Star.	Mag.	Cat.	Æ	D	Im.	Em.	Dist. Im.	Dist. Em.
				DECEMBER.					
				° ′	° ′	h m	h m	′	′
2	Piscium	7	L 8	21 45	13 13 N	3 34	4 22	3 S	4 S
3	Arietis	6·7	P 112	35 37	17 59	6 29	7 13	14 N	7 N
5	Tauri	7·8	L 11	62 33	22 28	5 2	5 47	4 S	10 S
—	———	7	L 13	64 27	23 6	8 34	9 31	10 N	4 N
—	———	7	L 13	64 40	22 53	9 0	9 50	7 S	12 S
—	———	6·7	L 13	68 16	23 14	16 41	17 25	9 S	9 S
7	μ Gemin.	3	P 74	92 43	22 36	5 26	6 3	8 N	8 N
—	———	7	L 9	96 37	22 12	12 37	13 39	8 S	4 S
—	———	7	L 9	98 48	21 54	17 4	17 58	2 N	8 N
8	———	8	L 13	106 7	20 24	6 25	6 55	12 N	15 N
—	———	7	L 13	109 18	20 3	8 8	9 0	6 S	4 S
—	———	7·8	L 13	109 30	20 15	8 33	9 10	11 N	14 N
—	81 ———	6	P 194	113 38	18 59	16 37	17 42	7 S	4 N
—	———	7	L 9	114 3	18 51	17 45	18 46	5 S	6 N
—	———	6·7	L 13	114 5	18 51	17 49	18 50	5 S	6 N
—	———	7	L 13	114 11	18 42	18 7	19 3	10 S	1 N
9	Cancri	7·8	P 106	126 13	16 0	12 26	12 54	13 N	17 N
10	6 Leonis	6	P 108	140 18	10 35	12 59	13 48	15 S	5 S
11	32 Sextan.	7	P 98	155 29	5 40	10 5	10 22	6 S	12 S
—	34 ———	6	P 138	158 4	4 37	15 4	16 10	3 N	16 S
—	———	7·8	L 10	158 18	4 19	16 22	17 7	16 S	6 S
—	———	6·7	L 10	158 32	4 24	16 28	17 39	9 S	7 N
12	Leonis	7	L 9	170 19	0 37 S	13 26	14 31	0	7 N
—	———	7·8	L 13	171 2	1 4	15 9	16 8	11 S	2 N
—	———	7·8	L 13	171 1	1 20	15 39	cont.		
13	Virginis	7·8	P 63	183 13	6 11	12 57	13 53	3 S	11 N
17	Scorpii	7	L 12	240 58	23 44	18 12	19 0	12 S	6 S
—	19 ———	5·6	P 46	242 9	23 40	20 13	21 1	8 N	13 N
18	θ Serpent.	3·4	P 53	257 26	24 47	21 25	22 24	6 N	1 S
25	Aquarii	7·8	L 10	333 47	6 14	6 35	7 47	10 N	4 S
26	———	7·8	L 10	344 24	1 38	5 32	6 51	8 N	8 S
—	———	7·8	L 10	344 15	1 26	6 7	cont.		
28	Piscium	7	L 8	7 35	8 12 N	9 6	cont.		
30	Arietis	7	L 10	29 28	16 14	4 21	5 7	13 N	4 N
31	———	7	P 261	44 28	19 59	8 37	9 37	4 S	11 S
—	58 ———	5	P 11	45 51	20 18	11 51	12 47	4 S	9 S

LXXVII. *Plantæ raræ Succulentæ; a Description of some rare Succulent Plants, by* A. H. Haworth, *Esq. F. L. S. &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

OBSERVING recently in your valuable Miscellany some Papers relating to Natural History, I am induced to send you the inclosed Descriptions of a few unrecorded Species of Succulent Plants, which, if acceptable, may be occasionally continued by

Your most obedient,

Queen's Elm, Chelsea, Nov. 1823. A. H. Haworth.

Cras-

CRASSULA. *Linn. &c.*

Concinnella. C. foliis obovatis, margine densissimè ciliatim ar-
1. genteo.

Species nova atque elegans a Capite Bonæ Spei. Affinis est Crassulæ concinnæ *nobis* in *Revis. Pl. Succ. p.* 200, at debilior, et in omnibus 4—5-plo minor, ciliis densioribus. *Folia* in ramulis subinde imbricata atque fere distichè compressa. Nondum in horto floruit. Communicavit amicus Dom. W. T. Aiton, A. D. 1822, e regio horto Kewensi.

KLEINIA. *Linn. in Hort. Cliff.—Nob. in Synops. Pl. Succ. p.* 312.

CACALIA. *Linn. &c.*

radicans. K. ramis erecto-decumbentibus grossè radicantibus,
2. foliis semiteretibus obtusis virescentibus, sæpe recurvis, supra altè canaliculatis.

Communicavit amicus Dom. Van Marum, ordinis Belgici Leonis eques, nomine Cacaliæ frutescentis; sed ab eâ abundè discrepat ramis numerosioribus crassioribus, radiculis numerosis grossis obtusis, longè supra terram (non erectis, sine radiculis), foliisque multoties majoribus. Forsan e Capite Bonæ Spei, floribus non mihi notis.

MESEMBRYANTHEMUM. *Linn. &c.*

mucroniferum. M. erectum, foliis obtusè farctim triquetris ca-
3. nescentibus mollibus mucronatis.

Communicavit cum sequente serenissimus Princeps De Salm Dyck, nomine "Mesemb. cymbiforme?" sed ab eo longè recedit, et magis affine Mesemb. *stricto Nob.*, cui simillimum, at majus, satisque differt, foliis remotioribus cum mucrone; quoque majoribus.

nobile. M. foliis grossè triquetro-clavatis subrecurvis, tuber-
4. culatim magnipunctatis: supra concavis. Mesemb. magnipunctum γ. *Nobis in Revis. Pl. Succ. p.* 87. *Habitat* C. B. S. *Floret* mense Novemb. G. H. ♃.

Obs. Folia infernè semiteretia, supernè auctim triquetra obtusa, at apicem versus sensim minus crassa. M. magnipunctnm *nobis* maxime simulat, at caudiculo altiore, foliis minus crassis, magis recurvis, minusque glaucescentibus; supra canaliculatim cavis nec planis ut in M. magnipuncto; magnipunctis tactu asperis (subelevatis) nec lævibus. *Flos* magnus foliis humilior antemeridianus inodorus sessilis luteus, basi grossè bibracteatus. Cætera non examinavi. Species bona atque insignis.

DACTY-

DACTYLANTHES. *Nob. in Synops. Pl. Succ. p.* 132.

globosa. D. subarticulato-prolifera, articulis variantibus, sæ-
5. peque sphæroideis.

Obs. Herba succulenta, præsingularis, nuper a Capite Bonæ Spei. Vidi florentem mense Octobris in Horto regio Kewensi, omnium in Succulentis hortorum longè ditissimo. Nullo valde affine, at parùm approximat Dactylanth. anacantham *Nob.*—Euphorbiam anacantham aliorum.

Flores longe pedunculati terminales subsolitarii fere ut in D. anacantha. Ramuli virides sæpe globosi, diametro semunciali plusve; sæpe (in Caldario) elongatim teretes, sive cylindrici, ut in D. anacantha. Species bona.

LXXVIII. *On the Cultivation of the English Cranberry* (Oxycoccus palustris) *in dry Beds. In a Letter to the Secretary. By Mr.* THOMAS MILNE, *F.H.S.**

SIR,

THE sample of English Cranberries which I had the honour of sending to the Horticultural Society on the 2d of September last, were gathered from cultivated plants growing on a bed made in the same way, in every respect, as for *Rhododendrons*, *Azalias*, *Andromedas*, and other plants generally denominated American. The soil was brought from Wimbledon Common, and was of that kind known by the name of black heath-mould, or peat, with a considerable quantity of white sand amongst it. The sand I however do not consider very essential to the growth of the *Oxycoccus palustris;* and, if we may judge from the soils on which it grows naturally, it would perhaps be as well, or better, without it. The plants were put into the bed in the spring at about one foot from each other every way; but I believe they would grow equally well, if planted at almost any other time of the year, except during the hot summer months, when there would be a greater risk of losing some of them, unless occasionally shaded and judiciously watered. As their slender shoots advanced, they were constantly laid into the ground about two or three inches deep, in order that they might the more certainly root, and be less influenced by the heat and dry weather in summer. This I consider of much importance, and am of opinion that it is in a great degree owing to that

* From the Transactions of the Horticultural Society of London, vol. v. Part III.

circumstance

circumstance that the plants have been so little affected by the extreme heat of the last summer. In two years the plants completely covered the bed, and last year (the third) they produced a crop of fruit which you had an opportunity of seeing. You then expressed an opinion that it might be desirable for the Horticultural Society to know the method of cultivating the English Cranberry so successfully on dry beds. But as the greater part of that season (1821) was singularly wet and cold, I was led to suppose that circumstance might have been the cause of their then making such vigorous shoots, and I therefore thought it better to suspend my opinion concerning them till I saw what effect a drier season would have on both the plants and fruit. The last, one of the hottest and driest I ever remember, afforded me the opportunity I wished for; and I have had the satisfaction to observe that the plants have continued nearly as vigorous, and the fruit has ripened as well, as in 1821, though a month earlier. As the produce was gathered at different times, to gratify the curiosity of ladies and gentlemen who visited our grounds in the course of the season, I cannot say exactly the quantity of fruit produced on a given space; but I think it was certainly not less than one quart on a bed five feet square, and I have no doubt, that, when the plants are more disposed by age to produce flowers and less vigorous shoots, the same space will yield a much greater crop. Some part of the bed is a little shaded by low pales, but how far that is a benefit to the plants, I do not pretend to say: last summer it became necessary to water all the American plants, and the Cranberry bed had an equal share with the rest, but not greater; in 1821 no artificial watering was necessary. The subsoil over which the bed is made is a sandy gravel, therefore not retentive of moisture, which is against the successful cultivation of this plant on dry beds; but where the soil is naturally moist or damp, with a free air, advantage might be taken of it, and the English Cranberry might be cultivated on it with much success. On a bed in a similar situation, and of the same sort of soil, the American Cranberry (*Oxycoccus macrocarpus*) grows most luxuriantly: but as a valuable paper on the cultivation of that species has been published in the Transactions of the Horticultural Society by Mr. Hallet*, I consider it unnecessary to add any thing to his directions and observations, which are plain, and, if followed, will be attended with success.

I have been long convinced that both species may be grown with much advantage in numberless situations in this

* Horticultural Transactions, vol. iv. page 483.

island,

island, and have been surprised that cottagers and others living on or in the neighbourhood of moors and heaths covered with soil suitable for their growth, have not been advised to cultivate them for the sake of profit. According to Withering's quotation from Lightfoot*, twenty or thirty pounds worth of the berries are sold by the poor people each market day for five or six weeks together, in the town of Langtown, on the borders of Cumberland. This is a considerable sum for berries picked up from barren wastes and in a district so thinly inhabited, and it is remarkable that the ready sale for them has not tempted some person to make the trial to supply the market in a more certain and regular way: if they could not be consumed or disposed of in the immediate neighbourhood, where they may be grown, they could easily be sent a great distance without the hazard of being spoiled. There is one very strong argument in favour of their cultivation, which is, that they may be made to grow with little trouble in places and on soils where few other useful plants yet known will grow to advantage. It may be said that the demand for them will be limited and uncertain; but that may have been said of a number of other things of a similar nature which now meet with a regular sale, and which the growers of course endeavour to cultivate according to the demand they have for them. If, to supply the whole of Great Britain, only the produce of one hundred acres were required, it would at least be one step towards making that quantity of waste land useful in some degree, and probably suggest some other improvement in various ways. Should any person be induced to make the trial, there can be no doubt the American Cranberry would be the easiest managed, and most productive for general use; but as many prefer the flavour of the English Cranberry, there would also be a demand for it on that account, though at a higher price.

I am, sir, your obedient servant,

Fulham, Dec. 10, 1822. THOMAS MILNE.

LXXIX. *Notices respecting New Books.*

Recently published.

PART II. of The Philosophical Transactions of the Royal Society of London, for 1823, has just appeared: the following are its contents:

On a new Phænomenon of Electro-magnetism: On the Application of Liquids formed by the Condensation of Gases

* Withering's Syst. Arr. of British Plants, 5th edition, vol. ii. p. 462.

as

as mechanical Agents. By Sir Humphry Davy, Bart.—On Fluid Chlorine: On the Condensation of several Gases into Liquids. By M. Faraday, Esq.—On the Motions of the Eye, in illustration of the Uses of the Muscles and Nerves of the Orbit. By Charles Bell, Esq.—An Account of an Apparatus on a peculiar Construction for performing Electro-magnetic Experiments. By W. H. Pepys, Esq.—On the Temperature at considerable Depths of the Caribbean Sea. By Captain Edward Sabine.—Letter from Captain Basil Hall to Captain Kater on Experiments made by him and Henry Foster, Esq., with an invariable Pendulum in London, and in different Parts of the Globe.—Account of Experiments made with an invariable Pendulum at New South Wales. By Major-General Sir Thomas Brisbane.—Observations and Experiments on the daily Variation of the horizontal and dipping Needles under a reduced directive Power. By Peter Barlow, Esq.—On diurnal Deviations of the horizontal Needle when under the Influence of Magnets. By S. H. Christie, Esq.—On Fossil Shells. By L. W. Dillwyn, Esq.—On the apparent Magnetism of metallic Titanium. By W. Hyde Wollaston, Esq.—An Account of the Effect of mercurial Vapours on the Crew of His Majesty's Ship Triumph, in the Year 1810. By Wm. Burnett, M.D.—On the astronomical Refractions. By J. Ivory, Esq.—Observations on Air found in the Pleura, in a Case of Pneumato-thorax. By John Davy, M.D.—On Bitumen in Stones. By the Right Hon. George Knox.—On certain Changes which appear to have taken place in the Positions of some of the principal fixed Stars. By John Pond, Esq.

Part III. of the Fifth Volume of the Horticultural Transactions has just been published. The following are its contents:

Observations on the Flat Peach of China: An Account of the injurious Influence of the Plum-stock upon the Moorpark Apricot: An Account of some *Mule* Plants: An Account of an improved Method of obtaining early Crops of Peas, after severe Winters. By Thomas Andrew Knight, Esq. President. —On the Cultivation of *Mesembryanthemums*. By Mr. William Mowbray.—On the Cultivation of the English Cranberry (*Oxycoccus palustris*) in dry Beds. By Mr. Thomas Milne.—On the Management of Cauliflower Plants, to secure good Produce during the Winter. By Mr. George Cockburn.—On the Cultivation of the *Tetragonia expansa*. By the Rev. John Bransby.—On a Method of securing the Scion when fitted to the Stock in grafting. By David Powell, Esq.—On the Woburn Perennial Kale, a variety of *Brassica oleracea*

 acephala

acephala fimbriata. By Mr. George Sinclair.—On the Cultivation of Horse-Radish. By Mr. Daniel Judd.—On a Method of cultivating the Mushroom. By Mr. William Hogan.—On the Fertilization of the Female Blossoms of Filberts. By the Rev. George Swayne.—On a Wash for Fruit-Trees. By John Braddick, Esq.—An Account of the Methods of forcing Peaches in Denmark and Holland. By Mr. Peter Lindegaard.—On the Modes now practised in Austria of cultivating Asparagus. By Mr. Jacob Baumann of Vienna.—A Notice of certain seedling Varieties of *Amaryllis*, presented to the Society by the Hon. and Rev. William Herbert, in 1820, which flowered in the Society's Garden in Feb. 1823. By Mr. John Lindley, Assistant Secretary at the Society's Garden.—On the Management of Fig-trees in the open Air. By Mr. Samuel Sawyer.—On the Cultivation of Melons in the open Air. By John Williams, Esq.—Description of an improved Pit for raising Cucumbers, Melons, and other Vegetables, by the Use of Steam instead of Stable Dung. By the Rev. William Phelps.—Description of *Amaryllis Psittacina-Johnsoni*, a new hybrid Variety raised by William Griffin, Esq. By James Robert Gower, Esq.—Description of a Method of protecting Cauliflower and other tender Plants during Winter. By Mr. James Drummond.

Works in the Press.

Dr. Hooker, the Professor of Botany at Glasgow University, is preparing a complete System of Plants, arranged according to the Natural Orders, with a Linnæan Index, and illustrated with numerous coloured Plates. One object of the author is to divest the study of Botany of the repelling feature of a dead language in which it has hitherto been clothed, by adopting our own instead of the Latin, and thus to promote the cultivation of the science throughout all classes of the community.

Mr. John Curtis has in the press the First Number of his Illustrations of English Insects. We understand the intention of the author is to publish highly finished Figures of such Species of Insects (with the Plants upon which they are found) as constitute the British Genera, with accurate representations of the parts on which the characters are founded, and descriptive letter-press to each plate, giving, as far as possible, the habits and economy of the subjects selected. The work will be published monthly, to commence the 1st of Jan. 1824.

Annals of Medical Botany and Pharmacy; to be published in Quarterly Numbers, edited by J. Frost, Professor of Botany and Materia Medica to the Medico-Botanical Society of London, &c.

LXXX. *Pro-*

LXXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

THE meetings of this Society were resumed, for the session, on Thursday, Nov. 20; when the Croonian Lecture, by Sir E. Home, V.P.R.S., was read, illustrative of the subject of muscular motion by the structure of the brain in man and various classes of animals, as microscopically examined and delineated by Mr. Bauer; a series of whose drawings were annexed. Part of a paperwas read, entitled, "Some Observations on the Migration of Birds," by the late Dr. Jenner, F.R.S., communicated by his nephew, the Rev. E. Jenner; and the remainder postponed to a future meeting. Major-gen. sir G. Murray, and John Rennie, Esq., were admitted Fellows of the Society.

Nov. 27.—The reading of Dr. Jenner's paper was concluded. Dr. Cresswell and Prof. Barlow were admitted Fellows of the Society.

LINNÆAN SOCIETY.

The first meeting of this Society, after the summer recess, was held on the 4th of November, A. B. Lambert, esq. V.P., in the chair. A great many valuable presents were on the table, consisting of works (chiefly foreign) on Natural History, and of 85 species of birds sent from the East Indies by Major-general Hardwicke, F.L.S., among which were a great many rare and several new species: with them were also the head of *Antelope quadricornis* (the Chikara of Bengal,) a description of which had been read to the Society on the 17th of last June, and a curious species of Musk Rat.

Two papers from W. Fothergill, esq., communicated by Dr. Sims, were read:—A Description of the Swallow-tailed Falcon, *Falco furcatus* Linn., taken near Hawes in Wensley Dale, Yorkshire, in 1805: and a Description of a Bird, supposed to be the *Rallus pusillus* of Latham, shot at the same place in 1807. Read also Observations on the Genus *Onchidium* of Buchanan, with a Description of a new Species; by the Rev. Lansdown Guilding, F.L.S. An improved generic character is given of *Onchidium*, belonging to the class *Mollusca*, order *Cephala*, div. *Gasteropoda:* "Corpus oblongum, repens, subtus planum. Penula carnosa pedem totum tegens. Os anticum, longitudinale. Anus posticus, infra. Tentacula duo retractilia. Oculi terminales."—Six species are enumerated, including a new one thus characterized: "*O. occidentale* dorso fusco, atomis brunneis elevatis sparsis, ventre pallido, lateribus livide-maculatis, brachiis apice divisis." Found in moist places of the mountainous parts of St. Vincent's.

Nov. 18.—The reading was commenced of a paper by John Murray.

Murray, esq. F.L.S., entitled "Experiments and Observations on the Light and Luminous Matter of the *Lampyris noctiluca*, or Glow-worm."

GEOLOGICAL SOCIETY.

Nov. 7.—A letter was read, dated May 10, 1823, from George Cumberland, esq. Hon. Mem. G.S., "On a Fossil of the Chalk," accompanied by a drawing.

A letter was read, dated July 14, 1823, from George Cumberland, esq. Hon. Mem. G. S., "On a new Species of Encrinus found in the Mountain Limestone near Bristol."

A notice was read, containing an Analysis of the Aluminite of St. Helena, by Dr. Wilkinson of Bath. Communicated by Col. Wilks, M.G.S.

On this analysis Col. Wilks observes, that there is a remarkable difference between the component parts of the aluminite of St. Helena, and the sub-sulphate of alumine found at Newhaven and Halle, as given by Phillips, page 111.

A paper was read "On the Geology of Parts of the Islands of Madeira, Porto Santo, and Baxo," by T. E. Bowdich, esq.

From the investigations of Mr. Bowdich, it appears that such parts of these islands as he examined, consist principally of horizontal strata of limestone and sandstone, containing fossils, intersected and sometimes capped by basalt.

Nov. 21.—An extract of a letter was read from the Rev. Lansdown Guilding, M.G.S., containing "An Account of a Fossil found in the blue Lias at the Berkeley Canal, near Gloucester," accompanied by the fossil.

A paper was read "On the Lias of the Coast in the Vicinity of Lyme Regis, Dorset." By H. T. De la Beche, esq. F.R.S. F.L.S. and M.G.S.

In a former communication in the first part of vol. i. second series of the Society's Transactions, the author had presented an outline of the geological features of the coast near Lyme Regis. The present paper is intended as supplementary, and the sections before published are referred to. Mr. De la Beche now enters into a detailed description, illustrated by a drawing, of the various strata composing the lias formation.

This formation consists of about 110 feet of lias, composed of more than 72 beds of limestones alternating with the same number of marl beds, surmounted by about 500 feet of lias marls. An account is subjoined of the various fossil shells, and other organic remains found in the lias, accompanied with several descriptive drawings.

ASTRONOMICAL SOCIETY.

Nov. 14.—This Society held its first meeting after the late recess this evening. Many valuable presents of books, &c. were

were received, and among these were an elegant inkstand for the Society's table, presented by an unknown donor, and some curious specimens of glass for object-glasses, made by a new process by a foreign artist, we believe residing at Neufchatel.

It was announced from the chair, that the Council had awarded the honorary Gold Medals of the Society to Charles Babbage, Esq. for his invaluable invention of applying mechanism to the purposes of calculation; and to Professor Encke for his investigations relative to the comet which bears his name.—Likewise similar Silver Medals to Mr. Rumker for the rediscovery of M. Encke's comet in 1822; and to M. Pons for the discovery of two comets in the same year, as well as for his indefatigable zeal in cometary astronomy.—Such medals to be presented at the anniversary of the Society in February next.

A paper by George Dollond, Esq. was read, descriptive of a new equal altitude instrument of his invention and construction; and the instrument was at the same time exhibited in the room. It is applicable to every purpose of measuring vertical angles or those in azimuth, and, by the adoption of two telescopes and an artificial horizon, affords no less than thirty-two readings of the same observation, if required; while the usual attention to adjustment of the levels of the instrument ceases to be necessary, on account of the object being seen by direct and reflected rays at the same time.

METEOROLOGICAL SOCIETY.

The second meeting of this Society took place on Wednesday, the 12th instant, pursuant to the resolutions given in our last Number, p. 306, and was numerously attended. Among the new members enrolled were Dr. Bostock, F.R.S., Messrs. H. T. Delabeche, F.R.S., J. F. Daniell, F.R.S., W. H. Pepys, F.R.S., &c. The Society proceeded to appoint a Council and Officers, a list of whom, with further particulars of the business of the evening, will be given in our next. Dr. Birkbeck was elected President.

LXXXI. *Intelligence and Miscellaneous Articles.*

MR. POND AND M. BESSEL.

A REPORT has been very generally and industriously circulated within the last two months, that M. Bessel has recently acknowledged, to a gentleman in this country, that there was an error in his *Catalogue* of the declination of the principal fixed

fixed stars, arising from the discovery of a *bending* in the telescope affixed to his meridian circle: and of such a magnitude as to account for all the discordances which existed between his observations and those of Mr. Pond.—We have nothing to do with the comparative merits of these two distinguished astronomers, or of the celebrated artists by whom their respective instruments were made: but, as we wish to guard the public, at all times, against *misrepresentation*, we think it our duty to *contradict* the above report; which we now do on the authority of the above-mentioned gentleman.

When M. Bessel, in the year 1820, received the present meridian circle from the hands of M. Reichenbach, he set to work immediately in the only proper way in which observations can be safely conducted: which was by endeavouring to *eliminate the errors of the instrument*. He minutely examined the divisions, and the centering of the circle, the form and regularity of the axes, and (what had not been done, we believe, by any former observer) instituted a series of observations in order to determine whether any error could arise from the bending of the telescope. All these points he investigated with his usual accuracy and ability; and the results have *long ago* been given to the world in various publications: but, the detail was reserved for the 7th part of his "Observations," where he has shown the steps of each process, and given tables and formulæ for correcting the errors arising from these sources.—It is needless to say that these corrections are properly applied in the formation of *his* catalogue, in the same manner as the *index-error*, or any other correctional error is applied in the formation of the *Greenwich* Catalogue: and the two catalogues can only be compared *as thus corrected*.

The error, arising from the bending of the telescope, he found, at a *maximum*, to be $1''.11$; as stated upwards of a twelvemonth ago in Bode's *Astronomische Jahrbuch*, and inserted in one of the former numbers of our Journal. But, this correction (he justly remarked) instead of reconciling the two catalogues, only *made the difference greater**. M. Bessel *has not communicated any particulars beyond these, nor any results more recent than those above mentioned; in fact, his correspondent was referred to the very works above alluded to, for a more full explanation of the subject of his letter*. And, it is evident that this correction of *one* second (which, by the by, *has been already applied* in the formation of his catalogue, and therefore, in this view of the subject, is *no error at all*) could never reconcile differences of *several* seconds which exist between the catalogue of Mr. Pond, and not only that of M. Bessel,

* See Phil. Mag. for January 1823, page 29.

but

but also those of other astronomers. In short, the catalogues of Mr. Pond himself, as published at various periods, *differ from each other* in some instances, by as large quantities as those above alluded to: and until the comparative catalogues assume a greater degree of consistency, and become more free from *oscillations*, it will be in vain to attempt to reconcile these fluctuating and minute discordances; which, after all, perhaps depend on circumstances *unconnected with the observer or the artist.*

It is fortunate, however, for the interests of astronomy, that the progress of the science is not arrested till those *minor* anomalies are adjusted. For, whilst we are amusing ourselves with these *minute* discussions, and commenting on the *ever-varying* comparisons, the continental astronomers are, with rapid strides, enlarging the bounds of the science, as well by their discoveries and observations, as by their numerous researches into various interesting points of physical and practical astronomy.

ASTRONOMICAL INFORMATION.

The indefatigable Bessel, to whom every branch of astronomy is so much indebted, is proceeding rapidly in his general *Survey of the heavens.* He has observed all the zones (with a very trifling exception) from $+15°$ to $-5°$ declination; and has made great progress in the survey of the zones contained between $-5°$ and $-15°$. He has already observed upwards of *twenty-five thousand* stars; amongst which are many new *double* stars. Five thousand of these stars observed in the year 1821, are given in the 7th number of his "Observations:" and the remainder will follow in succession. Dr. Struve of Dorpat, and M. Argelander at Abo, are associated in this grand undertaking, and will observe the more northerly stars. Those who are desirous and capable of distinguishing themselves in this laudable career, and are favourably situated for that purpose, would render an essential service to the science by observing the more southerly stars. It is only by an union of men of talent and enterprize that this splendid outline can be filled up.

The method, to which we alluded in a former number, of determining the difference of longitude between two observatories by a comparison of the culmination of the moon and certain stars *near* her at that time, is pursued with great success on the continent: and the practical astronomer, who is desirous of determining the longitude of his observatory, will do well to take advantage of this favourable circumstance. M. Schumacher, who is ever assiduous in promoting the best interests

interests of astronomy, has published (in his *Astron. Nach.*) a catalogue of all the stars that are near the moon at the time of her culmination, not only for the year 1824, but also for the year 1825, in order that distant observatories may take advantage of the method. M. Bouvard at Paris, M. Schumacher at Altona, M. Bessel at Konigsberg, M. Argelander at Abo, and Dr. Struve at Dorpat, are in the constant habit of making these comparisons and of recording their observations: so that distant observers may, by comparing those results with their own observations, easily deduce the longitude of their observatories. This method of determining the difference in the longitudes of two *distant* observatories is the best that has been hitherto proposed for that purpose; and is capable of considerable accuracy. M. Nicolai, M. Bessel, M. Hansen, and M. Mollweide, have distinguished themselves in the formation of correct formulæ and useful tables for deducing the required results.

M. Schumacher has just published his Astronomical Tables for the year 1824. We understand that they are conducted with the same ability and accuracy, and arranged nearly in the same manner, as the former ones: but they have not yet reached this country. An English preface will be prefixed; showing the nature and use of the tables.

M. Reichenbach, so celebrated for his astronomical instruments, with which most of the foreign observatories are furnished, has relinquished this branch of his profession, and removed to Vienna, where he is employed in the Imperial arsenal. He has invented a new method for the boring of cannon: but at present it has not succeeded to his wishes. Our own excellent artist, the unrivalled Troughton, still maintains his pre-eminence; and we even anticipate further proofs of his superior talents in the construction of some new circles now in contemplation. Long may he live, to enjoy the fruits of his well-earned fame!

The *Astronomische Jahrbuch* for 1826 is arrived in town, and contains the same variety of useful and interesting intelligence for which this work is so much distinguished.

The third volume of the observations made at the Imperial Observatory at Vienna, by M. Littrow, and the third volume of the Observations made at Dorpat by M. Struve, have also reached this country. This latter work contains some very curious and interesting observations and remarks on *double* stars: to the study of which, this distinguished astronomer has devoted a considerable portion of his time. The same author has also published a list of 795 double stars, arranged in the order of their right ascension: which will be useful to those who are fond of these researches.

LENGTH OF THE SECONDS' PENDULUM AT THE GALAPAGOS, IN MEXICO, AND IN BRAZIL.

It appears, from the details of experiments made with an invariable pendulum, by Capt. Basil Hall, F.R.S., and Mr. Henry Forster, as published in the Philosophical Transactions for 1823, part ii., that the length of the seconds' pendulum, at the volcanic island of Abingdon, one of the Galapagos, lat. 0° 32′ 19″ N., long. 90½ W., is 39.01717 inches; and the mean of all the ellipticities thereby deduced from Captain Kater's experiments in England, $\frac{1}{284.98}$, and from those of Captain Sabine at Melville Island, $\frac{1}{292.14}$.

They have also made two series of experiments at San Blas de California, a sea-port town on the N.W. of the coast of Mexico, lat. 21½ N., long. 105¼ W., and not far from the south point of California. By the first of these, the length of the seconds' pendulum at that place comes out 39.03776 inches, and the mean ellipticity $\frac{1}{313.53}$. By the second series, the length of the pendulum comes out 39.03881 inches, and the mean ellipticity $\frac{1}{308.36}$: "the circumstances in this case, however, were not so favourable as those of the first series, being to one another in the ratio of 47 to 397, or nearly as 1 to 8. This arose from the change which took place in the weather at that period, the sky being overcast, the temperature fluctuating, and the rate of the clock unsteady." Two extensive series of experiments were made at Rio de Janeiro, in Brazil, lat. 22° 52′ 22″ S., long. 43¼ W. By the first, the length of the pendulum is 39.04381 inches, and the mean ellipticity $\frac{1}{301.77}$: by the second series, deemed most satisfactory, the length of the pendulum is 39.04368, and the ellipticity $\frac{1}{302.37}$.

NEW VOYAGE PROJECTED BY CAPTAIN PARRY.

It will be recollected, that Captain Parry in his first voyage discovered, after entering Lancaster Sound, an opening, which he called Prince Regent's Inlet; leaving that, which seemed to turn to the south west, on his left hand, he proceeded in a north westerly direction. This Inlet promised well at the time, but the body of Lancaster Sound not having been then explored, it was passed by. We understand that the Admiralty, at the suggestion of Captain Parry, have resolved that this Inlet shall also be examined, in order that no opening which promises success may be neglected: he is therefore to proceed thither in the ensuing summer, in the Hecla, and from the situation where Hearne discovered the sea, and the apparent direction of Prince Regent's Inlet, he hopes to succeed in reaching Captain Franklin's Cape Turnagain through it. If the wished-for discovery should not be made in this direction, at least so en-

terprising an officer cannot be employed there without adding to our knowledge of regions which, before modern improvements had taught us to master the elements, were inaccessible to the inhabitants of temperate climates. From his perseverance, however, we may look forward with some confidence to this third voyage accomplishing its object, or making great approaches to its attainment.

GOLD MINES IN RUSSIA.

[From the *Conservateur Imperial* of Oct. 21.]

The senator, Mr. Soimonoff, and Dr. Fuchs, Professor of Medicine at the University of Cassan, have just made a journey to Mount Oural, which will promote the interests of science as well as those of the government. These two gentlemen visited the gold mines, which have been discovered within these three years. They have ascertained that the mines which are situated to the east of Mount Oural are much richer than those of the opposite side. The former extend from Verkhoturie as far as the source of the river Oural. But the place where the gold is found most abundantly is between Nijne Tajilskoi and Kousehtoumkoi, in a space of about 300 versts, cr 200 English miles. These mines are near the surface, and the golden earth is several *archines*, each archine is 28 inches in depth. The gold is obtained by washing the earth, and this labour is so easy that it is performed solely by boys. The metal is formed in separate grains, sometimes in large pieces or masses weighing six *marcs*. But in general five *zolotnics*, or about 15 pennyweights, are obtained from a hundred *pouds* of earth, or 5200 pounds troy;—the proportion being 1 in 83.200. A single proprietor, Mr. D. Jakowlaff, on whose estates the richest mines have been discovered, will send this year about 30 pouds, 1560 pounds troy, of gold to the mint at Petersburgh. The other mines of Oural will furnish altogether about 130 pouds, 6760 pounds troy. This is however only the commencement of working the mines.

Dr. Fuchs writes, that the gold appears to have been originally disseminated in the greenstone of Werner, with schistous talc, serpentine, and gray iron; and that these substances having been decomposed, have left the gold by itself. He adds in his letter, addressed to Mr. Magnitzky curator of the University of Cassan, that the mineral products of the mountains which he has visited are both rich and immense. Platina, adamantine spar, and other metals and valuable gems, both of India and America, are found there. Mr. Fuchs has made a discovery amongst the latter, viz. of a stone of the nature of the

the sapphire, to which he has given the name *soimonite* in honour of the learned mineralogist Mr. Soimonoff. There is no doubt that the University of Cassan will receive specimens of all these objects, which are as precious as they are novel, for its collection. But the advantages of the examinations and discoveries of Mr. Fuchs will not be confined to the University. This learned Professor means very soon to publish his journal to Mount Oural, which will contain not only his observations on the natural history of the country in general, but also the statistics of all that part which he has traversed and explored.

SUPPOSED GIGANTIC SPECIES OF RAIA.

From the President of the New York Lyceum of Natural History, to the Members, dated New York, September 11, 1823.

"On the 9th day of September, 1823, returned from a cruise off Delaware Bay, the fishing smack Una. She had sailed about three weeks before from New York, for the express purpose of catching an enormous fish, which had been reported to frequent the ocean a few leagues beyond Cape Henlopen. The adventurers in this bold enterprise have been successful. They have brought, for the enlargement of science and the gratification of curiosity, an uncommon inhabitant of the deep, which has never been seen on the land before. The creature is one of the huge individuals of the family of Raia; or perhaps may be erected, from its novelty and peculiarity, into a new genus, between that, the Squalus, and the Acipenser. Its strength was such, that after the body had been penetrated by two strong and well-formed gigs of the best tempered iron, the shank of one of them was broken off and the other singularly bent. The boat containing the three intrepid men, John Patchen, Theophilus Beebe, and William Porter, was connected, after the deadly instrument had taken hold, with the wounded inhabitant of the deep by a strong warp or line. The celerity with which the fish swam could only be compared to that of the harpooned whale, dragging the boat after it with such speed, as to cause a wave to rise on each side of the furrow in which he moved several feet higher than the boat itself. The weight of the fish after death was such, that three pair of oxen, one horse, and 22 men, all pulling together, with the surge of the Atlantic wave to help, could not convey it far to the dry beach. It was estimated from this (a probable estimate) to equal four tons and a half, or perhaps five tons. The size was enormous; for the distance from the extremity of one wing or pectoral fin to the other, expanded like the wing of an eagle, measures 18 feet; over the extremity of the back, and on the right line of the belly, 16 feet;

feet; the distance from the snout to the end of the tail, 14 feet; length of the tail, four feet; width of the mouth, two feet nine inches. The operation of combat and killing lasted nine hours. It was an heroic achievement, and was witnessed by crowds of citizens on the shores of New Jersey and Delaware, and by the persons on board the flotilla of vessels in the bay and offing. During the scuffle, the wings, side flaps, or vast alated fins of the monster lashed the sea with such vehemence that the spray rose to the height of 30 feet, and rained round to the distance of 50 feet. It was a tremendous encounter; on shore, all was awe and expectation. Mr. Patchen, whose taste and zeal in Zoology are well known, has attended very much to the manners of the *Vampire* of the Ocean; to the preservation of the skin and external parts; to the osteology and skeleton; the internal organizations; and, in short, to every circumstance that was practicable during such a hazardous business, and the tempestuous weather, which distressed them almost from the beginning to the end of their voyage. I merely mention, before I lay down my pen, that this animal is viviparous, and of course connects fishes with mammiferous animals; and that the respiratory motion, generative and sensitive organs, present an extraordinary amount of rare and interesting particulars. This is but an outline; I intend to finish this sketch; and prepare it as well as I can for the Society's formal notice.—SAMUEL L. MITCHILL."

PROF. DŒBEREINER'S NEW EXPERIMENTS.

The following additional particulars on this subject are derived from a paper by M. Dœbereiner, in the *Annales de Chimie*, tom. xxiv. p. 94—96.

Referring to his experiments on the 27th of July, 1819, (see p. 291 of our last number,) he says, "I found on that occasion, that by contact with the platinum powder, the combustible energy of hydrogen is so greatly augmented, that in a few minutes it will combine with all the oxygen of a mixture, containing but 1 part of that substance with 99 of nitrogen; which, as is well known, cannot be effected by the strongest electric sparks. For these experiments, however, I mix the platinum powder with potter's clay, and form a mixture by moistening it, into balls of the size of a pea; I leave these balls to dry in the air, and then heat them to a bright red with an enameller's lamp. Such a ball of platinum, although not weighing more than two, four, or six grains, is capable of converting into water any volume whatever of the detonating mixture, provided that after each experiment care be taken to dry it; and it may be employed for the same purpose above a thousand times.

The

"The combination with oxygen of the gaseous compounds of hydrogen, such as ammonia, olefiant gas, carburetted hydrogen, muriatic acid gas, &c. is not effected by platinum powder.

"When a jet of hydrogen is directed upon a mixture of platinum powder, and nitrate of platinum and ammonia, the mixture becomes ignited, with decrepitation, and the emission of inflamed sparks. The same effect takes place with the black powder, which is separated by zinc from solutions of platinum, and which is a mixture of protoxide of platinum and of the metal itself. This powder, with the aid of oxygen, has the property of gradually converting alcohol into acetic acid.

"Among the other metals which I have hitherto subjected to experiment, I have found the property of converting a mixture of hydrogen and oxygen into water, to belong only to nickel, as obtained by the decomposition of its oxalate; the effect in this case is produced very slowly."

MR. MURRAY ON THE MEDICAL APPLICATIONS OF CHLORINE AND CHLORATE OF POTASSA.

From the circumstance of chlorine elevating the temperature of the cutis, as Mr. Murray has already pointed out in the Philosophical Magazine, vol. lx. p. 61, 100, he is inclined to think the administration of a solution of chlorate of potassa to such persons as are labouring under that singular malformation of the heart, in which black and red blood intermingle and circulate through the body, would prove extremely beneficial; and it is his wish that this substance should be put to the test in some other diseases, for he has a strong conviction of its being worthy of a place in our Pharmacopœias. The utility of chlorine, so far from being founded on conjecture, has been experienced by himself: for instance, when he has accidentally lacerated his hand, by introducing it into a vessel of chlorine the wound has afterwards granulated and healed kindly. Moreover, by inhaling some of this gas in a state of dilution in atmospheric air, whilst under catarrhal inflammation, the cough and other severe symptoms have subsided. On this account, he would wish to offer chlorine to the notice of the medical world, as extremely likely to prove beneficial in cases of *phthisis pulmonalis* even, especially in its early stage. This formidable disease being connected with dyspepsia and scrofula, is further likely to meet with a considerable check, from his having found the preparations of chlorine favourable to the relief of dyspeptic symptoms. The nitro-muriatic or chlorine bath has been serviceable both in dyspepsia and scrofula. The application of chlorine in an aërial form requires caution and great circumspection: a small

small portion of oxide of manganese and muriatic acid in a cup floating in a bason of warm water, will soon sufficiently impregnate a room of small dimensions with the gas, and the due quantity may be determined by the feelings of the patient. Mr. Murray, in protesting against the hopelessness which seems to invade the minds of the medical profession in general, goes on to suggest the expediency of making trial of the preparations of the above-mentioned gas in that most dreadful of diseases, *rabies canina,* or hydrophobia. Brugnatelli of Pavia has recorded the cure of four persons in the hospital there, bitten by a rabid wolf. Aqueous solution of chlorine was administered in these cases. In Troillet's cases, however, solution of chlorine was of no use. In connexion with the history of hydrophobia, he gives the case of a woman who was bitten whilst in a state of pregnancy, but was not affected till several weeks after her delivery, when hydrophobic symptoms manifested themselves, and she very soon fell a victim to the malignant virus. A sow in farrow, two horses, and a dog, were bitten at the same time: the horses and dog died not long after, whilst the sow in farrow continued free from the disease till some weeks after she had farrowed, when the poison became fatally active. The offspring in both these cases were unaffected. The treatment Mr. M. would deem advisable in a case where hydrophobic phænomena were already manifested, would consist in administering the solution of chlorine or chlorate of potassa *internally,* and galvanism and the nitro-muriatic bath *externally,* besides exposing the patient to an atmosphere weakly charged with chlorine: this latter remedy appeared to have some effect in allaying the hydrophobic symptoms in a dog, although it was not resorted to till four days after the first attack of the disease. The animal subsequently died; but its death, Mr. M. thinks, might be owing rather to obstruction in the alimentary canal, than to the effects of the *rabies canina,* the obstruction being a consequence of the heterogeneous substances which the animal had swallowed in the incipient stage of the disease.

A letter from a medical gentleman of Derby accompanied Mr. M.'s paper, detailing several cases of the great efficacy of chlorate of potassa in uterine hæmorrhage and in hæmoptysis; the dose was about eight grains, and repeated every three or four hours, dissolved in water: but it must be observed that the treatment of these hæmorrhages was not confined to this remedy alone.

LIST OF NEW PATENTS.

To John Ranking, of New Bond-street, Westminster, Middlesex, esq., for his means of securing valuable property in mail and other stage coaches, travelling carriages, waggons, caravans, and other similar public and private vehicles,

vehicles, from robbery.—Dated 1st of November 1823.—2 months allowed to enrol specification.

To George Hawkes, of Lucas-place, Commercial-road, Stepney Old Town, Middlesex, ship-builder, for his improvement in the construction of ship anchors.—1st November.—6 months.

To George Hawkes, of Lucas-place, Commercial-road, Stepney Old Town, Middlesex, ship-builder, for certain improvements on capstans.—6 months.

To William Burdy, of Fulham, Middlesex, mathematical-instrument maker, for his anti-evaporating cooler to facilitate and regulate the refrigerating of worts or wash, in all seasons of the year, from any degree of heat between boiling and the temperature required for fermenting.—1st November.—6 months.

To Thomas Foster Gimson, of Tiverton, Devonshire, gentleman, who, in consequence of communications made to him by a certain person residing abroad, and of discoveries made by himself, is in possession of an invention for various improvements in addition to machinery now in use for doubling and twisting cotton, silk, and other fibrous substances.—6th November.—6 months.

To Thomas Gowan, of Fleet-street, London, truss-manufacturer, for certain improvements on trusses.—11th November.—2 months.

To John Day, of Barnstaple, Devonshire, esq., for certain improvements in percussion gun-locks applicable to various descriptions of fire-arms.—13th November.—2 months.

To John Ward, of Grove-road, Mile-End-road, Middlesex, iron-founder, for certain improvements in the construction of lock, and other fastenings. 13th November.—2 months.

To Samuel Sewill, of Brown's Hill, Bisley, Gloucestershire, clothier, for his new mode or improvement for dressing of woollen or other cloths.—13th November.—2 months.

To Richard Green, of Lisle-street, in the parish of St. Anne, Middlesex, sadlers' ironmonger, for certain improvements in constructing gambadoes or mud-boots, and attaching spurs thereto, and part of which said improvements are also applicable to other boots.—13th November.—2 months.

To Robert Stein, of the Tower Brewery, Tower-Hill, London, brewer, for his improved construction of a blast-furnace, and certain apparatus to be connected therewith, which is adapted to burn or consume fuel in a more œconomical and useful manner than has been hitherto practised.—13th November.—6 months.

To Joseph Gillman, of Newgate-street, London, silk warehouseman; and John Hewston Wilson, of Manchester, Lancashire, silk and cotton manufacturers; for certain improvements in the manufacture of hats and bonnets. —18th November.—6 months.

To John Heathcoat, of Tiverton, Devonshire, lace manufacturer, for a machine for the manufacture of a platted substance composed either of silk, cotton, or other thread or yarn.—20th November.—6 months.

To Thomas Hopper, of Reading, Berkshire, esq., for certain improvements in the manufacture of silk hats.—2d November.—6 months.

To Charles Anthony Deane, of Charles-street, Deptford, Kent, ship-caulker, for his apparatus or machine to be worn by persons entering rooms or other places filled with smoke, or other vapour, for the purpose of extinguishing fire, or extricating persons or property therein.—20th November.—6 months.

To Jacob Perkins, of Hill-street, London, and John Martineau the younger, of the city road, Middlesex, engineers, for their improvement in the construction of the furnace of steam-boilers and other vessels, by which fuel is œconomised, and the smoke is consumed.—20th Nov.—6months.

METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr. Burney at Gosport, Mr. Cary in London, and Mr. Veall at Boston.*

Days of Month, 1823.	Gosport, at half-past Eight o'Clock, A.M. Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Clouds. Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Height of Barometer, in Inches, &c. Lond. 1 P.M.	Bost. $8\frac{1}{2}$ A.M.	Thermometer. London. 8 A.M.	Noon.	11 P.M.	Boston. $8\frac{1}{2}$ A.M.	Rain. London.	Boston.	Weather. London.	Boston.
Oct. ☾ 26	30·44	49	$53\frac{1}{4}$	63	NW.	...	......	...	...	1	...	...	...	...	30·47	30·24	46	47	45	48	...	...	Cloudy	Cloudy
27	30·28	50	...	63	NW.	...	......	...	1	1	...	...	...	...	30·25	30·04	45	54	48	47·5	...	...	Cloudy	Cloudy
28	29·86	52	...	63	S.	0·10	0·175	...	...	1	...	...	1	1	29·80	29·56	50	52	50	46	...	...	Foggy	Cloudy, rain *p.m.*
29	29·72	47	...	67	W.	...	240	1	...	1	...	1	...	1	29·72	29·35	45	49	44	43	...	·15	Fair	Fine
30	29·23	47	...	80	NE.	...	1·520	...	...	...	...	...	1	1	29·18	29·20	42	46	42	41·5	...	...	Rain	Rain
31	28·98	41	$52\frac{3}{4}$	74	N.	·10	·120	...	...	1	...	...	1	1	29·16	29	42	42	40	40	...	·90	Rain	Rain and Stormy
Nov. 1	29·74	40	...	71	N.	...	......	...	1	1	...	...	...	...	29·85	29·61	39	45	37	42·5	2·00	1·61	Showery	Cloudy
● 2	30·06	34	...	65	N.	...	......	1	...	1	...	1	...	...	30·12	29·95	33	46	32	33	...	...	Fair	Fine
3	30·02	41	...	66	NE.	·08	·095	1	...	1	...	...	...	1	29·95	29·83	35	50	49	36	...	...	Cloudy	Fine
4	29·66	51	...	72	S.	...	·220	...	...	1	...	...	1	1	29·63	29·35	50	51	47	48	...	...	Cloudy	Rain
5	29·60	47	...	80	E.	...	·300	...	...	1	...	...	...	1	29·70	29·50	45	50	49	45	...	·11	Rain	Rain
6	29·86	54	$52\frac{3}{4}$	84	SE.	·08	·540	1	...	1	...	1	...	1	29·93	29·70	50	56	48	50	...	·41	Fair	Cloudy
7	29·94	53	...	80	NW.	...	......	1	1	1	...	1	...	...	30·01	29·65	48	56	49	51	...	...	Cloudy	Rain
8	30·13	49	...	78	NW.	...	......	...	...	1	...	...	...	1	30·23	29·93	49	51	44	49·5	1·10	·37	Cloudy	Rain
9	30·35	45	...	62	NE.	·15	......	...	1	1	...	...	...	...	30·44	30·25	40	45	41	45	...	.03	Fair	Cloudy
☽ 10	30·46	44	...	63	NE.	...	......	...	...	1	...	1	...	...	30·52	30·35	40	44	38	43·5	...	...	Fair	Cloudy
11	30·57	38	...	60	NE.	...	......	1	...	1	...	...	...	...	30·65	30·43	35	40	35	38	...	...	Cloudy	Fine
12	30·42	37	52	65	E.	·35	......	1	...	1	...	...	...	...	30·45	30·26	30	42	34	33	...	...	Fair	Fine
13	30·40	46	...	61	SE.	...	......	1	...	1	...	...	...	...	30·44	30·24	32	46	35	30	...	...	Fair	Fine
14	30·34	33	...	71	NW.	...	......	1	1	1	...	...	...	...	30·25	30·10	33	43	43	43	...	...	Fair	Cloudy
15	30·24	47	...	72	N.	·10	......	...	...	1	...	...	...	...	30·30	30·05	45	50	45	46	0·00	...	Fair	Cloudy
16	30·38	47	...	64	NE.	...	......	...	...	1	...	...	1	...	30·44	30·25	43	47	36	38·5	...	...	Fair	Misty
17	30·36	43	...	68	N.	...	......	...	...	1	...	...	...	...	30·40	30·17	36	42	45	43	...	...	Cloudy	Misty
○ 18	30·38	44	...	70	NE.	·10	......	...	...	1	...	...	1	...	30·46	30·25	45	48	44	44	...	...	Cloudy	Fine
19	30·18	47	52	67	NE.	...	......	1	1	1	...	...	...	...	30·18	30	44	46	45	43	...	...	Fair	Cloudy
20	30·10	50	...	72	W.	...	......	...	...	1	...	...	1	...	30·06	29·70	46	52	44	46	...	...	Cloudy	Cloudy
21	30·20	47	...	72	W.	·05	......	...	...	1	...	...	...	...	30·16	29·90	45	50	47	45·5	...	...	Fair	Fine
22	29·96	47	...	71	SE.	...	......	...	...	1	...	...	...	...	29·98	29·70	46	49	45	45	0·00	...	Cloudy	Fine, rain *p.m.*
23	29·99	47	...	71	S.	...	·015	...	...	1	...	...	...	1	30·03	29·75	45	48	47	46	...	·15	Cloudy	Cloudy
24	30·00	47	...	76	E.	...	......	1	1	1	...	...	...	1	30·07	29·77	47	51	47	47·5	...	...	Cloudy	Cloudy
☾ 25	30·21	45	52	76	W.	·07	......	...	...	1	...	...	1	...	30·25	29·95	46	52	48	49	...	...	Cloudy	Cloudy

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

31st *DECEMBER* 1823.

LXXXII. *The Specific Characters of several undescribed Shells. By* W. Swainson, *Esq. F.R. & L.S. &c.**

To the Editors of the Philosophical Magazine and Journal.

PURSUANT to the intention which I expressed in a former Number of your Magazine (vol. lxi. p. 376), I now proceed to describe the characters of several rare shells which have recently come under my inspection, and of others which I am led to think have not been well understood.

1. Strombus *Thersites*.

S. testâ ponderosâ; anfractu basali gibbo, deformi; spirâ attenuatâ, tuberculatâ; labii exterioris integri margine crasso, inflexo, recto, aperturâ lævi.

Shell ponderous, body whorl gibbous, deformed; spire attenuated, tuberculated; outer lip entire, the margin thick, inflexed, and straight; aperture smooth.

Size and habit of *S. Accipiter;* but the spire is longer in proportion, the body whorl destitute of grooves, or compressed nodules, and the outer lip is considerably inflexed, very thick, and attached to the first spiral whorl. The back is swelled and has a distorted appearance. It is a large and exceedingly rare shell. Of the two specimens I have met with, one is in the Cracherodian collection of the British Museum; and, if I am not mistaken, is stated in the MS. catalogue to be a native of New Caledonia: the other is in the cabinet of Mr. Broderip under the name by which I have recorded it.

2. Strombus *galeatus*.

S. testâ magnâ, ventricosâ, inermi, transversim sulcatâ; spirâ brevissimâ; labio exteriore integro, suprà rotundato, dilatato, in spiram ascendente.

Shell large, ventricose, unarmed, transversely grooved; spire very short; outer lip entire, above rounded, dilated, and ascending on the spire.

This *Strombus* has long been known to collectors in its young

* Communicated by the Author.

state; but two or three adult specimens have recently been brought to this country from the coast of Peru. Its size is nearly equal to *Strombus Gigas*, but in appearance it resembles a *Dolium;* the outer lip is dilated only on its upper part. Mr. Broderip is in possession of the finest specimen I have yet seen, and he suggested to me the very appropriate name which I have here given it.

3. STROMBUS *integer*.

S. testâ nodosâ; labii exterioris subinflexi, suprà obliquè rotundati, integri, ad spiram annexi, margine externo recto; aperturâ lævi, albâ.

Shell nodulous; outer lip sub-inflexed, above obliquely rounded, entire, attached to the spire, with the exterior margin straight; aperture smooth, white.

Resembling in habit *S. Accipiter*, but is smaller, and the spire more lengthened. The exterior margin of the outer lip, instead of being curved outwards, is perfectly straight. This shell has long existed in my father's collection, but my recent possession of another specimen has removed the doubts I had entertained of its being a distinct species.

4. UNIO *cuneatus*.

U. testâ transversim cuneatâ, anticè obliquè truncatâ; dentibus lateralibus brevissimis, crenatis.

Shell transversely wedgeshaped; anterior side obliquely truncate; lateral teeth very short, crenated.

Inhabits North America. Mus. nost.

This *Unio* (for such, notwithstanding its peculiar form, I consider it to be) is totally unlike any other species yet discovered. If the shell is placed perpendicularly, so that it rests on the posterior end, it presents the perfect appearance of a thick wedge. It is a small species; the cardinal teeth resemble those of *U. pictorum;* and the lateral teeth (from the abrupt truncation of the anterior side) are very short.

5. AMPULLARIA *conica*.

A. testâ ovato-globosâ, lævi; basi contractâ; spirâ crassâ, productâ, conicâ; umbilico obsoleto, basali; aperturæ margine sulcato; operculo testaceo.

Shell ovato-globose, smooth, base contracted; spire thick, produced, conic; umbilicus obsolete, placed near the base; margin of the aperture grooved; operculum testaceous.

The umbilicus in this *Ampullaria* is quite closed, and is situated nearer to the base of the aperture than that of any other species. The spire also is thicker and more elevated. It is usually of a beautiful olive green colour without bands. The operculum of a specimen before me is testaceous.

6. ANCILLA *rubiginosa.*

A. testâ ovatâ, fusiformi, glabrâ, rufâ; spirâ elongatâ et aperturâ longitudine eâdem gaudentibus; basi sulcis tribus canaliculatis scabrâ.

Shell ovate, fusiform, smooth, rufous; spire produced, as long as the aperture; base with three channelled grooves.

The prolongation of the spire forms the distinguishing character of this species, which is of the greatest rarity.

7. LINGULA *anatina* of authors.

L. testâ depressâ; dorso corrugato; basis dilatatæ extremitatibus divaricatis.

Shell depressed; the back wrinkled; base dilated, the extremities diverging.

8. LINGULA *hians.*

L. testâ subdepressâ, convexâ, dorso tantùm non lævi; basis contractæ extremitatibus hiantibus.

Shell sub-depressed, convex, the back nearly smooth; base contracted, the extremities gaping.

The belief that two distinct shells had hitherto been confounded under the common name of *L. anatina*, first struck me when examining the magnificent collection of Lord Tankerville; and the observations I have since made, and the numerous specimens I have examined, have both tended to strengthen this belief. I have therefore here assigned to each its specific character; and have only to observe, in this place, that the species to which I have retained the original name is that which has been so ably described by Cuvier. That extremity of the shell where the fleshy peduncle is attached, I have termed the base; although it might perhaps with equal propriety be termed the umbones; in one species the valves at this extremity approach very near each other; but in *L. hians* they are widely gaping.

9. PATELLA *nigra.*

P. testâ depressâ, ovatâ, nigrâ, sub-glabrâ; vertice ad marginem anticum approximante; margine interno lævi, nigricante; disci albentis parte anticâ maculâ fulvâ tinctâ.

Shell depressed, oval, black, nearly smooth; summit very near the anterior margin; margin within smooth, blackish; disk whitish, the eye fulvous.

A very flat and remarkably large species; its shape is perfectly oval with a few obsolete striæ; the apex slightly incurved, and very near the margin within the rim of the shell is a border of black.

Inhabits California. Mr. Mawe.

 LXXXIII. *On*

LXXXIII. *On the Management of Cauliflower Plants, to secure good Produce during the Winter.* *By Mr.* George Cockburn, *Gardener to* W. S. Poyntz, *Esq.**

I HAVE the honour to send herewith a head of Cauliflower, which I shall be happy to have laid before the Horticultural Society, as I believe it has seldom been exceeded in size and beauty at this season of the year; and at the same time I trouble you with a brief account of my mode of cultivation, which, if you think proper, may accompany it.

I sow the seeds of the Early Cauliflower in a south border, in the beginning of July; and as soon as the plants come up, I thin them out to twelve or fourteen inches apart, where I suffer them to remain, keeping them clean, and watering them occasionally, till about the middle of November, by which time they all produce heads from ten to thirty inches in circumference. As they are not hardy enough to bear more than three or four degrees of frost, I remove them at that time into a shed which will keep out ten degrees of frost, taking care to retain as much mould about their roots as possible, and to remove all their decayed leaves. In the shed they are planted in mould, keeping a space of about an inch between each head. In this state they are frequently looked over with care, their dead leaves removed, and those heads cut for present use which show any disposition to decay. When severe frost occurs, the plants are covered with dry short hay. By this management I have been able to send three dishes of Cauliflowers to the table every week during the autumn and winter, till the present time, and shall be able to continue to do so until February.

I am
Your most obedient servant,
George Cockburn.

Cowdray Lodge, Sussex,
Jan. 13, 1823.

Note by the Secretary.—The head of Cauliflower above alluded to, was sent to the house of the Society the day after the meeting of the 14th of January. It was nearly thirty inches in circumference, very compact, and of good colour. It boiled tender, and was of excellent flavour.

* From the Transactions of the Horticultural Society, vol. v. Part III.

LXXXIV. *Description of a Method of protecting Cauliflower and other tender Plants during Winter.* By Mr. James Drummond*.

My success for several years past in *protecting cauliflower plants, in earthen pits, from frost and snow, during winter*, by means of wooden frames covered permanently with straw, induces me to send an account of the plan to the Horticultural Society.

My pits are mostly made in a south and east border, in an inclosure or yard which I have for hot beds, composts, &c., the fences of which afford good shelter from the cold quarters. To form the pits, I first make the ground as level as I can, and as firm as possible, by trampling in wet weather, I then cut them out ten feet in length by four in breadth, making the sides and ends as firm as possible by beating the soil when wet with the spade. The depth of the pit is according to the description of plants to be kept in them. Nine inches is sufficient for cauliflower plants, and for these care must be taken that a sufficient quantity of proper soil is left, or placed in the bottom of the pit in which they are to be pricked out. Each pit of the above dimensions holds about four hundred cauliflower plants. For plants in pots the depth of the pits must be proportioned to the height of the plants, the tops of which must, when placed in the pits, be below the level of the surface of the ground.

The frames proper to cover these pits are twelve feet in length by six in breadth; I prefer them of that, to a larger size, for such can be conveniently carried where wanted between two men, and can be easily opened and shut, to give light and air to the pits, by a single person.

The timbers to form the sides and ends of the frames are required to be about three inches square, and quite straight. These, when joined together, are placed on a level floor, and slips of timber two inches in breadth and one in thickness are nailed lengthways on them at intervals of nine inches. When the timber work is finished, the straw is fastened on in layers in the manner of thatch, and tied to the bars by rope yarn. The straw used is what is called in this country *reed*; it is prepared by taking the wheat in handfuls out of the sheaf, and beating it against a door firmly fixed on edge: by this method of threshing, the straw is very little bruised except at the points, and is consequently preferred for thatching.

* From the Transactions of the Horticultural Society, vol. v. Part III.

The

The frames are always kept under shelter in summer, being perfectly dried before they are put up, and with proper care will last for several years.

When the plants are put into the pits, the frames are laid over them. My method of giving air is by placing in the ground, near the centre of each pit, a forked stick about four feet or more in length, strong enough to support the frames when raised like the lid of a box, to a sufficient height, and they remain in that position night and day, unless when actual freezing takes place, or when frost is expected in the night.

I am far from thinking that these straw frames will bear a comparison with glass for neatness of appearance; but they have other advantages besides their cheapness: when they are raised, the plants in the pits have the full advantage of air and sun, and are but little exposed to wet, the rain being mostly thrown off on the back of the frames; and when they are shut down, frost cannot easily penetrate through them to the plants.

It is well known that it is necessary to have mats and other sorts of coverings over glass in severe weather, the removing of which to give air in the middle of the day, and replacing at night, is attended with much trouble; whereas the opening and shutting of the straw frames is but the work of a moment.

I have principally used these pits and frames for the protection of Alpine and other plants usually kept under glass without fire heat; but in cases of necessity tender green-house plants may be preserved through the winter in them, as I experienced last season. I had many Geraniums and other tender plants which I could not find room for in the glass houses. By way of experiment I placed them in these pits; and although, from the unusual severity of the winter, I was obliged to keep down the frames night and day for a fortnight together, and cover them with additional straw to exclude the severe frost, the only plants that suffered were a few of the downy-leaved Geraniums, and even those, on being planted afterwards in the ground, shot out vigorously in the spring at every joint. I have often tried to keep Geraniums in hot-bed frames through the winter, but could never succeed, if the weather was severe.

I am, &c.

Botanic Garden, Cork,
May 12, 1823.

JAMES DRUMMOND.

LXXXV. *Ob-*

Fig. 4

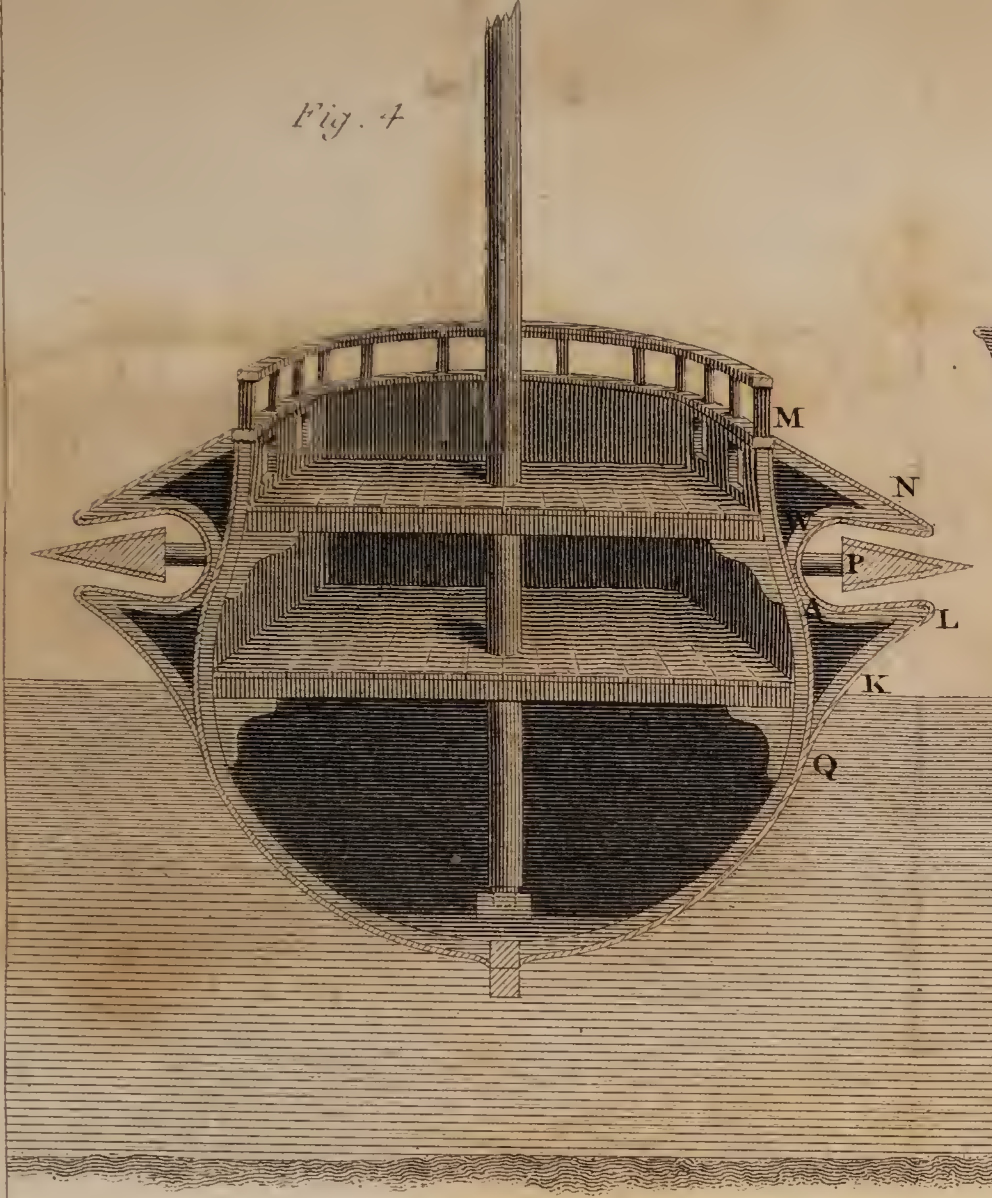


Fig. 5

Fig. 3

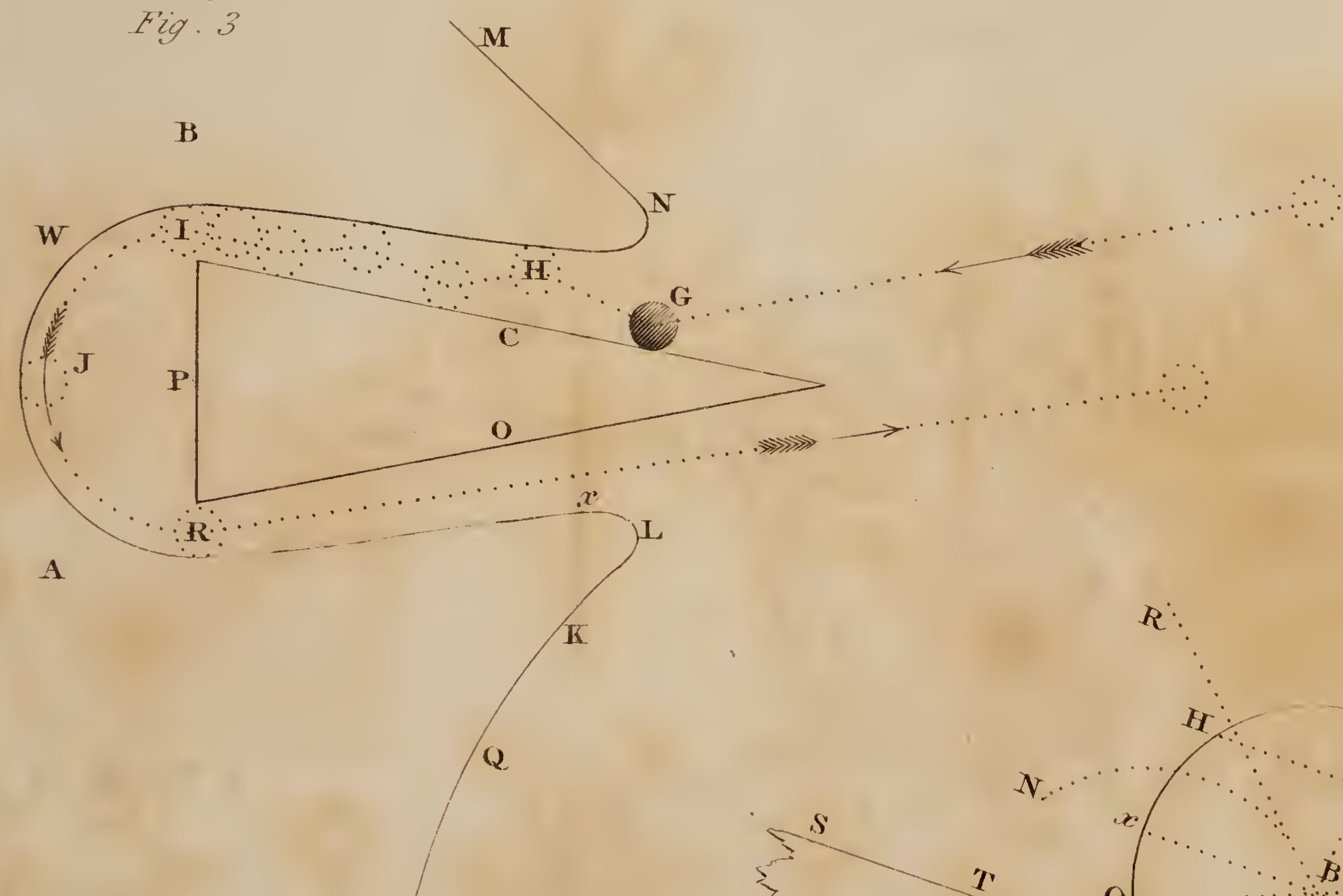


Fig. 2

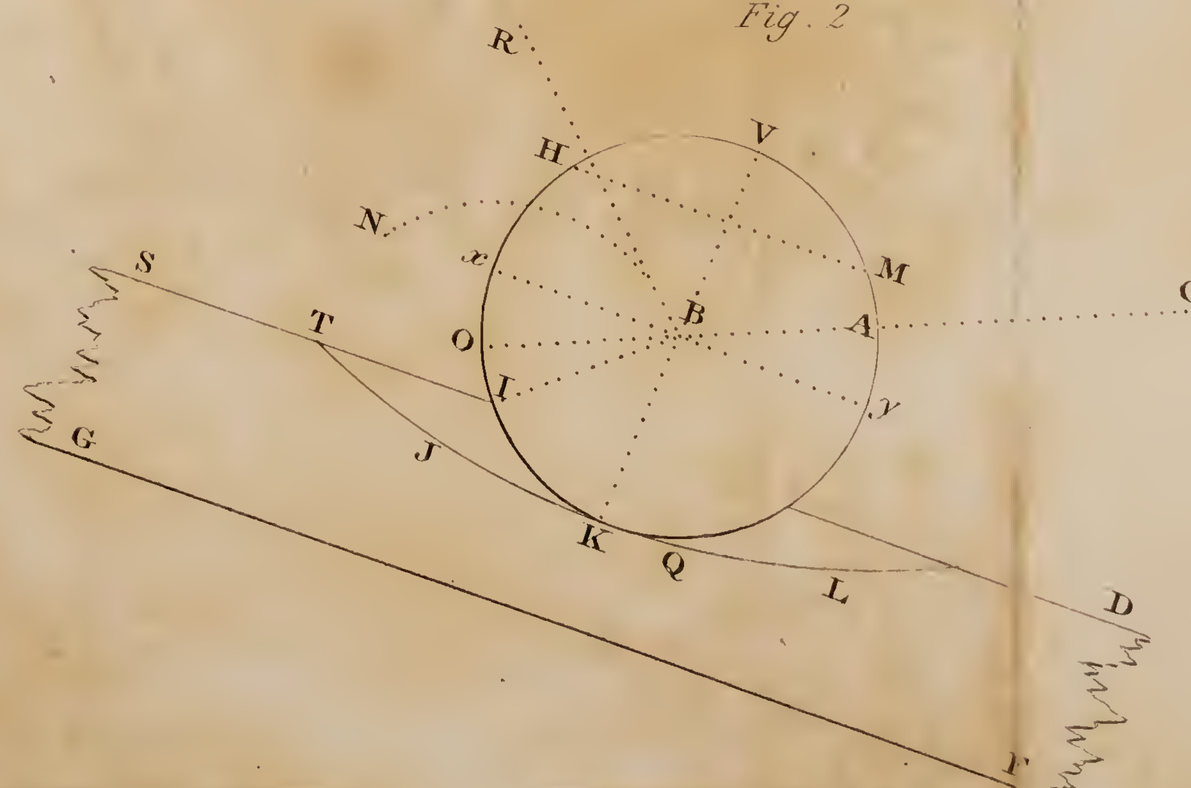
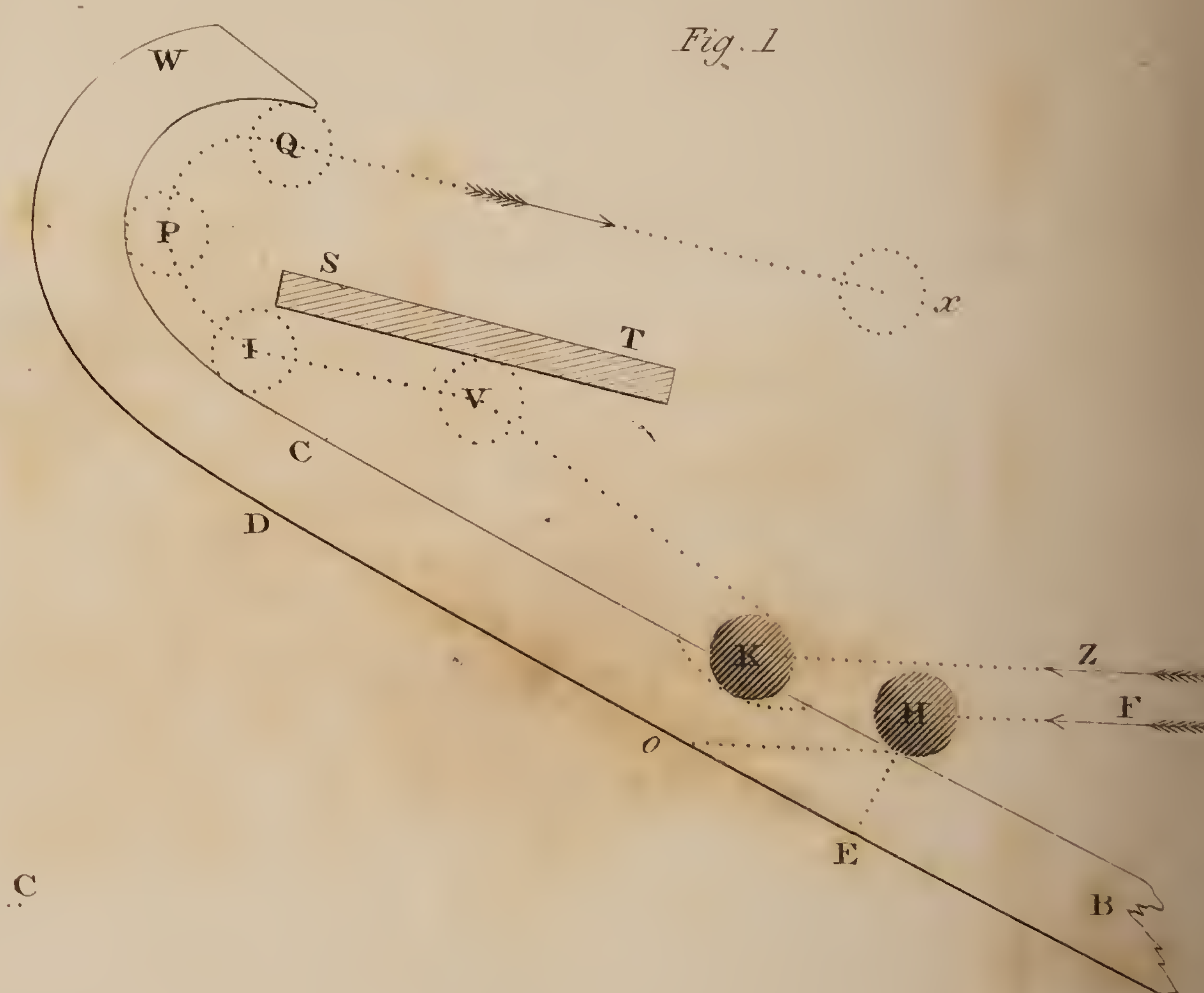


Fig. 1



LXXXV. *Observations concerning a Method of defending Ships and Fortifications against Cannon Balls, and of causing them to fly back again on the Enemy.* By Lewis Gompertz, *Esq.**

HAVING made some experiments on a plan which I had designed for rendering ships and fortifications shot-proof, and of causing several of the balls which might be fired against them to return upon the enemy; and having found my experiments, which were on a small scale, to answer my expectations, I have here to explain the nature of the plan, with the hopes that it may be further considered by those whose scientific and practical information qualifies them for judging how far it might succeed on a large scale.

But before I enter into this description, I think it proper to observe that the chief utility it may promise, is in its application to merchant vessels, ships of passage, &c., and for fortifications; but for ships of war (as it could be adopted by both parties) its effect would become *neutralized*, though it seems that even in this case it would save the men from injury, and would always be in favour of the weak and defensive side; its nature being that of defending itself and of returning the blows, but without any power of attacking, unless furnished with guns also.

Figs. 4 and 5 show two views of a ship made on the plan. Fig. 3 is a section of a side drawn larger, the form of it being apparent by the drawing, in the three figures 3, 4 and 5; the same letters refer to the same parts. NWAL is a concave curve to return the balls which strike it: and PCO is a triangular piece (extending beyond N M and L K) which goes all round the ship to protect the most perpendicular part of the curve WA from being struck directly (otherwise it would be easily perforated), and which triangular piece, on being struck somewhat horizontally, evades the balls, and guides them properly to the return part NWAL, so that they follow the shape of it, and return. The part NM above the curve where the port-holes are, and the part LKQ below it, are made oblique, to evade those balls which strike them, the part NM sending them upwards, and the part LKQ directing them into the water, though it must be confessed that some of the former would thereby occasionally be thrown into the rigging; there are a number of supports shown near P, fig. 4, and also faintly expressed in fig. 5, which fasten the triangular piece to the ship, and the more acute the outward angle be, the less force will it generally be struck with.

* Communicated by the Author.

Fig.

Fig. 1 is also a section of a side of a different construction, but inferior, and less applicable, though being more simple and on nearly the same principles, I will describe the nature of that first, or rather both together; the same reasoning applying to each. BC is the side forming an acute angle with the water, and extending some way under the water, but not far, as balls do not generally penetrate that part of a ship which is far below the surface of the water; ST is a board placed as shown, so that there shall be a vacancy existing between itself and the side of the ship; this vacancy grows progressively less upwards, till there is only room left for a ball to pass, and the board is fastened by different supports in places to the ship, but these are not put in this figure, as they would hide the operation. The part W is so curved as to return the balls after they have struck the inclined part; but as in this construction the return part might be struck by balls coming directly against it, without their having struck the inclined part, it might be required to make the most perpendicular place of it near P strong enough to *resist* the balls, this portion of the curve being very small. The effects then will vary in different cases, and will depend on the *hardness* and on the *elasticity* of the material of the side of the ship, and of the ball; also on the force with which the balls are fired: the following results, it seems, would then be produced.

Case 1. If the ball and the side were perfectly elastic, and of sufficient hardness not to be broken, or if *only* the side were perfectly elastic, then, according to the established law, the ball would be reflected backwards and forwards in fig. 1, between the side CB and board ST, and in fig. 3 between IC and IH, at equiangles, and would *not* follow the shape of the curve; and if the force of the ball should not be too much destroyed by the operation, it would at last be reflected off, though most likely not in a proper direction to reach the enemy.

2dly. If neither the side nor the ball should possess any elasticity, and the side were perfectly hard, whether the ball should be hard, or whether it should be soft (so as to indent), it would be turned out of the direction, and would in fig. 1, if struck at H, proceed up the inclined side BC, and would follow the shape of the curve W (the motion of the centre being shown dotted at IPQ), and it would then return to X and in fig. 3, if it should strike at H, it would proceed in the direction of the whole shape NWAL (the motion of the centres being shown dotted at HIJR); and it would return as the arrows point: but if, in fig. 1, it should strike at V, or in fig.

fig. 3 at G, the respective balls would, after sliding or rolling up the boards TS fig. 1, and IC fig. 3, strike each of the curves in such a direction as to follow their shapes and return, without any reflection taking place; and in fig. 3, those balls which entered at G would return at X, and *vice versâ*.

Case 2.—If the force of the ball K, fig. 1, should only be so far evaded by the inclination of the side, as to penetrate to about half the depth of its own size or less (shown large at *xy*, fig. 2); and if there were no elasticity in the substances; there would, it seems, then arise a great force to repel the ball beyond what is immediately caused by the inclination of the side, on account of the rotary motion the ball would have acquired by its action against the inside of the indentation: thus suppose BAHQ, fig. 2, be a section of the ball going nearly in a parallel direction CB, and suppose IKLQ be the indentation, in which place we will fancy the substance of the side to be so hard as not to give way any more, the effect, it seems, would then be, that the centre of the ball B would *begin* to describe part of a circle BN, about the centre I (the point where the indentation and the remainder of the side meet, and of the size of the ball itself). Then if the indentation should be deep, and the velocity great, the ball would be forced completely out of it, and fly far above the top of the ship, because the part of the circle BN, which the centre B of the ball would *begin* to describe, would be nearly perpendicular to the side SD; and as there would be nothing to change the direction of the ball after it has once acquired this new motion, it would fly off in the direction of the most perpendicular part of the circle BN, and continue in this direction, though not of the *continued* circle BN, but in a straight line BR: if, however, the indentation should be small, the line BR would be more nearly parallel to the side SD, in which case the motion of the ball would not be caused to differ so much from the direction of the side, but that it might strike the flat board TS, fig. 1, in a direction KV, which would prevent it from flying away and direct it to the side again, so that it followed the return part and flew back again, after having been reflected backwards and forwards, not by means of any elasticity, but by the reaction of the inside surface of the indentation against the ball (as before described); and as there would be a loss of force at every blow, each indentation would be less than the preceding one, and each angle of reflection would be more obtuse, as is shown in fig. 3, till the ball arrived at the return part WA, so as to follow the shape of it, ceasing sensibly to rebound when the indentation ceased sensibly to take place: but as the indentation and point I

would

would not be so hard as assumed, the effect would not be exactly as described; though as there would be a continual tendency for it to be so, according to the hardness of the side, it would be produced to a certain degree, and the ball would accordingly continually widen the indentation, and come out at some other point T, instead of I (fig. 2); and as the new direction would, by the yielding of the substance, be less perpendicular than when the material was extremely hard, the ball would be the more inclined to follow the curvature of the side, and to return, and the less inclined to fly over the top of the ship; as the angles of reflection would thereby become still more obtuse every time that the indentations it would produce in its course would widen, as just alluded to.

It seems that the tendency of being reflected by the reaction of the indentation would exist in some degree till the ball was completely buried, allowing the material of the side of the ship to be as deep as the ball; because, suppose the ball to be partially buried to HM, fig. 2, (above the diameter,) and allowing even that it should still be as much inclined to go in its original direction CB as it was at first (though it is evident that it must have acquired some tendency to alter its direction by the blow, &c.), then, to see this clearly, to the diameter xy, which is parallel to the side of the ship, draw another diameter VK perpendicular to it; it will be obvious that as the ball continued to penetrate, it would be opposed at its whole buried surface HK; and it is also plain that, if the resistance to the part of the ball between x and K tended to press the ball *upwards*, resistance above this line between x and V would tend to bury it still deeper: but as the whole of the arc xK would be greater than part of the arc xV (HV being by hypothesis unburied), arc xV would always cause most resistance; there would consequently be more than a balance of force to press it upwards, which would exist till the ball was wholly buried, but would then cease.

But both in this case and in case 1, the ball has two modes of acting, either in going up the side, or out of the indentation, that is, by *rolling* or *sliding*, both of which would rob the ball of some of its force, by the friction produced; but the less should be the impediments which cause friction, the more would it be inclined to slide, and the more of them there should be, the more would it be inclined to *roll* in its course; but even this would also rob the ball of some of its progressive force, and would be spent in giving a new motion (of rotation) to it, which would assist it to roll up the side of the ship or to roll out of the indentation: but it must be particularly observed, that either the rotary motion of the ball, or the

the action of its curved surface in sliding, would tend to force the centre of the ball out of the indentation in the same manner, as it is easy to be perceived that the centre would describe the same curve if it were to roll or to slide.

It is moreover evident that *this* power of turning the ball from its direction would be added to *that* derived immediately from the obliquity of the side, though this would be the cause of it all; or, in other words, the effect would be different (whatever was the hardness of the side) from what it would be if the ball were a mere point or flat body acting against another flat oblique surface or indentation.

Case 3. If the force should be so great that the ball entirely buried itself, there would even then be two circumstances in favour of this construction; first, that the ball would have to perforate through a greater substance than if the side were perpendicular to the motion, the distance of which is shown at HE, fig. 1, and the oblique distance shown at HO; and secondly, because the change of motion which would take place before the ball was quite buried (as above described) would still further increase the length of substance to be perforated by it, and the course of the ball might be so much changed that it should (after it was quite sunk) have to perforate the side through the remainder of its length upwards, instead of through its direct thickness.

It would be possible, however, for the balls to come in a perpendicular direction to the side, and to go through it directly; but it is improbable that this should frequently be the case, and it seems that it would be less likely to happen if it were fired at from a short distance than from a great one, as then only a moderate elevation would be required; whereas, when the distance was small, the elevation of the guns would become so great that it would be extremely difficult to take an aim so that the balls should come down upon it.

It remains to be observed, that neither of the cases would exist altogether as described; but as all substances possess a certain degree of hardness and elasticity, there would be a mixed effect produced, though I do not conceive the elasticity of wood to be sufficiently great to alter the cases materially: the results would therefore, it appears, be nearly as stated when the elasticity was not supposed to exist, but with some very sensible difference.

I have also to add, that since having made the preceding observations, I tried the experiments relative to them on a small scale, and found them precisely according to my ideas. The side, fig. 1, was represented by a *deal* board, 3-8ths of an inch thick; the return part W was of plate-iron, and the

 inside

inside of the board ST was (perhaps improperly) coated with iron; the bullets were of lead, and about one-third part of the weight of a musket-ball, and they were fired from a blunderbuss well charged. They made very slight long dents not 1-8th of an inch deep in the deal; and when the additional board TS was not used, they flew upwards and perforated the iron return part W; but when the board TS was added, they each made a dent also near the narrow part I, and followed the return part W; and then they returned against a deal board placed behind the stock of the blunderbuss, and left moderately deep impressions on it.

I also made experiments on the plan of figs. 3, 4, 5, though on a still smaller scale, and on a model which was made of deal, but with the same accordant results to the remarks I have made.

I rather think that a thin coating of iron on a wooden side would not be advantageous, as the iron would bend away from that part of the ball which it should be in absolute contact with, and the ball would then be improperly directed: therefore, whatever substance be employed, it should be of such a nature as to fit the ball as it goes, and the grain of the wood, of which the side &c. is made, should be in the direction of the motion of the ball, *not* transversely.

It is scarcely necessary to notice, that if the object of returning the ball be dispensed with, the side may simply be formed into a triangle C O P, fig. 3, 4 and 5, without the concave part NWAL; and in fig. 1, without the return part W, and board TS, though the balls would be thrown more into the rigging by this means.

The lower part L should perhaps rather bend upwards to cause the balls to fly a little upwards in returning, because those which come against the side H, fig. 3, 4 and 5, will not only be lowered the whole distance between H and L, but, as they will return rather more slowly than they came, they will also be attracted downwards with more force by the power of gravity; the curve will likewise be more effective if made smaller at the entry NL, than at the other part WA. I have also to add that a coating of grease on the side &c. is, it seems, of service.

But I am fully aware, that however the experiments might have succeeded in miniature, the great force of a cannon ball might defy them all, though it is known that slight obstructions affect their motion when opposed to them obliquely. It also remains to be further tried, whether the balls would be returned with sufficient force.

Any person repeating these experiments should (in order to

to avoid danger) stand at the side of the gun at a great distance, and tie a string to the trigger, and of course must not place himself either behind or before it.

These observations are meant also to apply to fortifications, where it seems that the plan would be as effectual, or more so than for ships.

LXXXVI. *On Fluid Chlorine.* *By Mr.* Faraday, *Chemical Assistant in the Royal Institution. Communicated by Sir* H. Davy, *Bart. Pres. R.S.**

It is well known that before the year 1810 the solid substance obtained by exposing chlorine, as usually procured, to a low temperature, was considered as the gas itself reduced into that form; and that Sir Humphry Davy first showed it to be a hydrate, the pure dry gas not being condensible even at a temperature of $-40°$ F.

I took advantage of the late cold weather to procure crystals of this substance for the purpose of analysis. The results are contained in a short paper in the Quarterly Journal of Science, vol. xv. Its composition is very nearly 27·7 chlorine, 72·3 water, or 1 proportional of chlorine, and 10 of water.

The President of the Royal Society having honoured me by looking at these conclusions, suggested, that an exposure of the substance to heat under pressure would probably lead to interesting results; the following experiments were commenced at his request. Some hydrate of chlorine was prepared, and, being dried as well as could be by pressure in bibulous paper, was introduced into a sealed glass tube, the upper end of which was then hermetically closed. Being placed in water at 60°, it underwent no change; but when put into water at 100°, the substance fused, the tube became filled with a bright yellow atmosphere, and, on examination, was found to contain two fluid substances: the one, about three-fourths of the whole, was of a faint yellow colour, having very much the appearance of water; the remaining fourth was a heavy bright yellow fluid, lying at the bottom of the former, without any apparent tendency to mix with it. As the tube cooled, the yellow atmosphere condensed into more of the yellow fluid, which floated in a film on the pale fluid, looking very like chloride of nitrogen; and at 70° the pale portion congealed, although even at 32° the yellow portion

* From the Philosophical Transactions for 1823, Part II.

did

did not solidify. Heated up to 100° the yellow fluid appeared to boil, and again produced the bright coloured atmosphere.

By putting the hydrate into a bent tube, afterwards hermetically sealed, I found it easy, after decomposing it by a heat of 100°, to distil the yellow fluid to one end of the tube, and to separate it from the remaining portion. In this way a more complete decomposition of the hydrate was effected, and, when the whole was allowed to cool, neither of the fluids solidified at temperatures above 34°, and the yellow portion not even at 0°. When the two were mixed together, they gradually combined at temperatures below 60°, and formed the same solid substances as that first introduced. If, when the fluids were separated, the tube was cut in the middle, the parts flew asunder as if with an explosion, the whole of the yellow portion disappeared, and there was a powerful atmosphere of chlorine produced; the pale portion on the contrary remained, and when examined, proved to be a weak solution of chlorine in water, with a little muriatic acid, probably from the impurity of the hydrate used. When that end of the tube in which the yellow fluid lay was broken under a jar of water, there was an immediate production of chlorine gas.

I at first thought that muriatic acid and euchlorine had been formed; then, that two new hydrates of chlorine had been produced; but at last I suspected that the chlorine had been entirely separated from the water by the heat, and condensed into a dry fluid by the mere pressure of its own abundant vapour. If that were true, it followed, that chlorine gas, when compressed, should be condensed into the same fluid, and, as the atmosphere in the tube in which the fluid lay was not very yellow at 50° or 60°, it seemed probable that the pressure required was not beyond what could readily be obtained by a condensing syringe. A long tube was therefore furnished with a cap and stop-cock, then exhausted of air and filled with chlorine, and being held vertically with the syringe upwards, air was forced in, which thrust the chlorine to the bottom of the tube, and gave a pressure of about 4 atmospheres. Being now cooled, there was an immediate deposit in films, which appeared to be hydrate, formed by water contained in the gas and vessels, but some of the yellow fluid was also produced. As this however might also contain a portion of the water present, a perfectly dry tube and apparatus were taken, and the chlorine left for some time over a bath of sulphuric acid before it was introduced. Upon throwing in air and giving pressure, there was now no solid film formed, but the clear yellow fluid was deposited, and more abundantly still

still upon cooling. After remaining some time it disappeared, having gradually mixed with the atmosphere above it, but every repetition of the experiment produced the same results.

Presuming that I had now a right to consider the yellow fluid as pure chlorine in the liquid state, I proceeded to examine its properties, as well as I could when obtained by heat from the hydrate. However obtained, it always appears very limpid and fluid, and excessively volatile at common pressure. A portion was cooled in its tube to 0°; it remained fluid. The tube was then opened, when a part immediately flew off, leaving the rest so cooled, by the evaporation, as to remain a fluid under the atmospheric pressure. The temperature could not have been higher than −40° in this case; as Sir Humphry Davy has shown that dry chlorine does not condense at that temperature under common pressure. Another tube was opened at a temperature of 50°; a part of the chlorine volatilised, and cooled the tube so much as to condense the atmospheric vapour on it as ice.

A tube having the water at one end and the chlorine at the other was weighed, and then cut in two; the chlorine immediately flew off, and the loss being ascertained was found to be 1·6 grain: the water left was examined and found to contain some chlorine: its weight was ascertained to be 5·4 grains. These proportions, however, must not be considered as indicative of the true composition of hydrate of chlorine; for, from the mildness of the weather during the time when these experiments were made, it was impossible to collect the crystals of hydrate, press, and transfer them, without losing much chlorine; and it is also impossible to separate the chlorine and water in the tube perfectly, or keep them separate, as the atmosphere within will combine with the water, and gradually re-form the hydrate.

Before cutting the tube, another tube had been prepared exactly like it in form and size, and a portion of water introduced into it, as near as the eye could judge, of the same bulk as the fluid chlorine; this water was found to weigh 1·2 grain; a result, which, if it may be trusted, would give the specific gravity of fluid chlorine as 1·33; and, from its appearance in and on water, this cannot be far wrong.

Note on the Condensation of Muriatic Acid Gas into the liquid Form. By Sir H. Davy, *Bart. Pres. R.S.*

In desiring Mr. Faraday to expose the hydrate of chlorine to heat in a closed glass tube, it occurred to me, that one of three things would happen: that it would become fluid as a hydrate;

hydrate; or that a decomposition of water would occur, and euchlorine and muriatic acid be formed; or that the chlorine would separate in a condensed state. This last result having been obtained, it evidently led to other researches of the same kind. I shall hope, on a future occasion, to detail some general views on the subject of these researches. I shall now merely mention, that by sealing muriate of ammonia and sulphuric acid in a strong glass tube, and causing them to act upon each other, I have procured liquid muriatic acid: and by substituting carbonate for muriate of ammonia, I have no doubt that carbonic acid may be obtained, though in the only trial I have made the tube burst. I have requested Mr. Faraday to pursue these experiments, and to extend them to all the gases which are of considerable density, or to any extent soluble in water; and I hope soon to be able to lay an account of his results, with some applications of them that I propose to make, before the Society.

I cannot conclude this note without observing, that the generation of elastic substances in close vessels, either with or without heat, offers much more powerful means of approximating their molecules than those dependent upon the application of cold, whether natural or artificial: for, as gases diminish only about $\frac{1}{480}$ in volume for every degree of Fahrenheit's scale, beginning at ordinary temperatures, a very slight condensation only can be produced by the most powerful freezing mixtures, not half as much as would result from the application of a strong flame to one part of a glass tube, the other part being of ordinary temperature: and when attempts are made to condense gases into fluids by sudden mechanical compression, the heat, instantly generated, presents a formidable obstacle to the success of the experiment; whereas, in the compression resulting from their slow generation in close vessels, if the process be conducted with common precautions, there is no source of difficulty or danger; and it may be easily assisted by artificial cold in cases when gases approach near to that point of compression and temperature at which they become vapours.

LXXXVII. *On the Condensation of several Gases into Liquids. By Mr.* Faraday, *Chemical Assistant in the Royal Institution. Communicated by Sir* Humphry Davy, *Bart. Pres. R.S.**

I HAD the honour, a few weeks since, of submitting to the Royal Society a paper on the reduction of chlorine to the

* From the Philosophical Transactions for 1823, Part II.

liquid

liquid state. An important note was added to the paper by the President, on the general application of the means used in this case to the reduction of other gaseous bodies to the liquid state; and in illustration of the process, the production of liquid muriatic acid was described. Sir Humphry Davy did me the honour to request I would continue the experiments, which I have done under his general direction, and the following are some of the results already obtained:

Sulphurous Acid.

Mercury and concentrated sulphuric acid were sealed up in a bent tube, and, being brought to one end, heat was carefully applied, whilst the other end was preserved cool by wet bibulous paper. Sulphurous acid gas was produced where the heat acted, and was condensed by the sulphuric acid above; but when the latter had become saturated, the sulphurous acid passed to the cold end of the tube, and was condensed into a liquid. When the whole tube was cold, if the sulphurous acid were returned on to the mixture of sulphuric acid and sulphate of mercury, a portion was reabsorbed, but the rest remained on it without mixing.

Liquid sulphurous acid is very limpid and colourless, and highly fluid. Its refractive power, obtained by comparing it in water and other media with water contained in a similar tube, appeared to be nearly equal to that of water. It does not solidify or become adhesive at a temperature of 0° F. When a tube containing it was opened, the contents did not rush out as with explosion, but a portion of the liquid evaporated rapidly, cooling another portion so much as to leave it in the fluid state at common barometric pressure. It was however rapidly dissipated, not producing visible fumes, but producing the odour of pure sulphurous acid, and leaving the tube quite dry. A portion of the vapour of the fluid received over a mercurial bath, and examined, proved to be sulphurous acid gas. A piece of ice dropped into the fluid instantly made it boil, from the heat communicated by it.

To prove in an unexceptionable manner that the fluid was pure sulphurous acid, some sulphurous acid gas was carefully prepared over mercury, and a long tube perfectly dry, and closed at one end, being exhausted, was filled with it; more sulphurous acid was then thrown in by a condensing syringe, till there were three or four atmospheres; the tube remained perfectly clear and dry; but on cooling one end to 0°, the fluid sulphurous acid condensed, and in all its characters was like that prepared by the former process.

A small gauge was attached to a tube in which sulphurous

acid

acid was afterwards formed, and at a temperature of 45° F. the pressure within the tube was equal to three atmospheres, there being a portion of liquid sulphurous acid present: but as the common air had not been excluded when the tube was sealed, nearly one atmosphere must be due to its presence; so that sulphurous acid vapour exerts a pressure of about two atmospheres at 45° F. Its specific gravity was nearly 1·42.*

Sulphuretted Hydrogen.

A tube being bent, and sealed at the shorter end, strong muriatic acid was poured in through a small funnel, so as nearly to fill the short leg without soiling the long one. A piece of platinum foil was then crumpled up and pushed in, and upon that were put fragments of sulphuret of iron, until the tube was nearly full. In this way action was prevented until the tube was sealed. If it once commences, it is almost impossible to close the tube in a manner sufficiently strong, because of the pressing out of the gas. When closed, the muriatic acid was made to run on to the sulphuret of iron, and then left for a day or two. At the end of that time, much proto-muriate of iron had formed, and on placing the clean end of the tube in a mixture of ice and salt, warming the other end if necessary by a little water, sulphuretted hydrogen in the liquid state distilled over.

The liquid sulphuretted hydrogen was colourless, limpid, and excessively fluid. Ether, when compared with it in similar tubes, appeared tenacious and oily. It did not mix with the rest of the fluid in the tube, which was no doubt saturated, but remained standing on it. When a tube containing it was opened, the liquid immediately rushed into vapour; and this being done under water, and the vapour collected and examined, it proved to be sulphuretted hydrogen gas. As the

* I am indebted to Mr. Davies Gilbert, who examined with much attention the results of these experiments, for the suggestion of the means adopted to obtain the specific gravity of some of these fluids. A number of small glass bulbs were blown and hermetically sealed; they were then thrown into alcohol, water, sulphuric acid, or mixtures of these, and when any one was found of the same specific gravity as the fluid in which it was immersed, the specific gravity of the fluid was taken: thus a number of hydrometrical bulbs were obtained; these were introduced into the tubes in which the substances were to be liberated; and ultimately, the dry liquids obtaiued, in contact with them. It was then observed whether they floated or not, and a second set of experiments were made with bulbs lighter or heavier as required, until a near approximation was obtained. Many of the tubes burst in the experiments, and in others difficulties occurred from the accidental fouling of the bulb by the contents of the tube. One source of error may be mentioned in addition to those which are obvious, namely, the alteration of the bulk of the bulb by its submission to the pressure required to keep the substance in the fluid state.

temperature of a tube containing some of it rose from 0° to 45°, part of the fluid rose in vapour, and its bulk diminished; but there was no other change: it did not seem more adhesive at 0° than at 45°. Its refractive power appeared to be rather greater than that of water; it decidedly surpassed that of sulphurous acid. A small gauge being introduced into a tube in which liquid sulphuretted hydrogen was afterwards produced, it was found that the pressure of its vapour was nearly equal to 17 atmospheres at the temperature of 50°.

The gauges used were made by drawing out some tubes at the blow-pipe table until they were capillary, and of a trumpet form; they were graduated by bringing a small portion of mercury successively into their different parts; they were then sealed at the fine end, and a portion of mercury placed in the broad end; and in this state they were placed in the tubes, so that none of the substances used, or produced, could get to the mercury, or pass by it to the inside of the gauge. In estimating the number of atmospheres, one has always been subtracted for the air left in the tube.

The specific gravity of sulphuretted hydrogen appeared to be 0·9.

Carbonic Acid.

The materials used in the production of carbonic acid, were carbonate of ammonia and concentrated sulphuric acid; the manipulation was like that described for sulphuretted hydrogen. Much stronger tubes are however required for carbonic acid than for any of the former substances, and there is none which has produced so many or more powerful explosions. Tubes which have held fluid carbonic acid well for two or three weeks together, have, upon some increase in the warmth of the weather, spontaneously exploded with great violence; and the precautions of glass masks, goggles, &c., which are at all times necessary in pursuing these experiments, are particularly so with carbonic acid.

Carbonic acid is a limpid colourless body, extremely fluid, and floating upon the other contents of the tube. It distills readily and rapidly at the difference of temperature between 32° and 0°. Its refractive power is much less than that of water. No diminution of temperature to which I have been able to submit it, has altered its appearance. In endeavouring to open the tubes at one end, they have uniformly burst into fragments, with powerful explosions. By inclosing a gauge in a tube in which fluid carbonic acid was afterwards produced, it was found that its vapour exerted a pressure of 36 atmospheres at a temperature of 32°.

It may be questioned, perhaps, whether this and other si-

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milar fluids obtained from materials containing water, do not contain a portion of that fluid; inasmuch as its absence has not been proved, as it may be with chlorine, sulphurous acid, cyanogen, and ammonia. But besides the analogy which exists between the latter and the former, it may also be observed in favour of their dryness, that any diminution of temperature causes the deposition of a fluid from the atmosphere, precisely like that previously obtained; and there is no reason for supposing that these various atmospheres, remaining as they do in contact with concentrated sulphuric acid, are not as dry as atmospheres of the same kind would be over sulphuric acid at common pressure.

Euchlorine.

Fluid euchlorine was obtained by inclosing chlorate of potash and sulphuric acid in a tube, and leaving them to act on each other for 24 hours. In that time there had been much action, the mixture was of a dark reddish brown, and the atmosphere of a bright yellow colour. The mixture was then heated up to 100°, and the unoccupied end of the tube cooled to 0°; by degrees the mixture lost its dark colour, and a very fluid ethereal looking substance condensed. It was not miscible with a small portion of the sulphuric acid which lay beneath it; but when returned on to the mass of salt and acid, it was gradually absorbed, rendering the mixture of a much deeper colour even than itself.

Euchlorine thus obtained is a very fluid transparent substance, of a deep yellow colour. A tube containing a portion of it in the clean end, was opened at the opposite extremity; there was a rush of euchlorine vapour, but the salt plugged up the aperture: whilst clearing this away, the whole tube burst with a violent explosion, except the small end in a cloth in my hand, where the euchlorine previously lay, but the fluid had all disappeared.

Nitrous Oxide.

Some nitrate of ammonia, previously made as dry as could be by partial decomposition, by heat in the air, was sealed up in a bent tube, and then heated in one end, the other being preserved cool. By repeating the distillation once or twice in this way, it was found, on after examination, that very little of the salt remained undecomposed. The process requires care. I have had many explosions occur with very strong tubes, and at considerable risk.

When the tube is cooled, it is found to contain two fluids, and a very compressed atmosphere. The heavier fluid on examination proved to be water, with a little acid and nitrous oxide

oxide in solution; the other was nitrous oxide. It appears in a very liquid, limpid, colourless state; and so volatile that the warmth of the hand generally makes it disappear in vapour. The application of ice and salt condenses abundance of it into the liquid state again. It boils readily by the difference of temperature between 50° and 0°. It does not appear to have any tendency to solidify at −10°. Its refractive power is very much less than that of water, and less than any fluid that has yet been obtained in these experiments, or than any known fluid. A tube being opened in the air, the nitrous oxide immediately burst into vapour. Another tube opened under water, and the vapour collected and examined, it proved to be nitrous oxide gas. A gauge being introduced into a tube, in which liquid nitrous oxide was afterwards produced, gave the pressure of its vapour as equal to above 50 atmospheres at 45°.

Cyanogen.

Some pure cyanuret of mercury was heated until perfectly dry. A portion was then inclosed in a green glass tube, in the same manner as in former instances, and being collected to one end, was decomposed by heat, whilst the other end was cooled. The cyanogen soon appeared as a liquid: it was limpid, colourless, and very fluid; not altering its state at the temperature of 0°. Its refractive power is rather less, perhaps, than that of water. A tube containing it being opened in the air, the expansion within did not appear to be very great; and the liquid passed with comparative slowness into the state of vapour, producing great cold. The vapour, being collected over mercury, proved to be pure cyanogen.

A tube was sealed up with cyanuret of mercury at one end, and a drop of water at the other; the fluid cyanogen was then produced in contact with the water. It did not mix, at least in any considerable quantity, with that fluid, but floated on it, being lighter, though apparently not so much so as ether would be. In the course of some days, action had taken place, the water had become black, and changes, probably such as are known to take place in an aqueous solution of cyanogen, occurred. The pressure of the vapour of cyanogen appeared by the gauge to be 3·6 or 3·7 atmospheres at 45° F. Its specific gravity was nearly 0·9.

Ammonia.

In searching after liquid ammonia, it became necessary, though difficult, to find some dry source of that substance; and I at last resorted to a compound of it which I had occasion

sion to notice some years since with chloride of silver *. When dry chloride of silver is put into ammoniacal gas, as dry as it can be made, it absorbs a large quantity of it; 100 grains condensing above 130 cubical inches of the gas; but the compound thus formed is decomposed by a temperature of 100° F. or upwards. A portion of this compound was sealed up in a bent tube and heated in one leg, whilst the other was cooled by ice or water. The compound thus heated under pressure fused at a comparatively low temperature, and boiled up, giving off ammoniacal gas, which condensed at the opposite end into a liquid.

Liquid ammonia thus obtained was colourless, transparent, and very fluid. Its refractive power surpassed that of any other of the fluids described, and that also of water itself. From the way in which it was obtained, it was evidently as free from water as ammonia in any state could be. When the chloride of silver is allowed to cool, the ammonia immediately returns to it, combining with it, and producing the original compound. During this action a curious combination of effects takes place: as the chloride absorbs the ammonia, heat is produced, the temperature rising up nearly to 100°; whilst a few inches off, at the opposite end of the tube, considerable cold is produced by the evaporation of the fluid. When the whole is retained at the temperature of 60°, the ammonia boils till it is dissipated and re-combined. The pressure of the vapour of ammonia is equal to about 6·5 atmospheres at 50°. Its specific gravity was 0·76.

Muriatic Acid.

When made from pure muriate of ammonia and sulphuric acid, liquid muriatic acid is obtained colourless, as Sir Humphry Davy had anticipated. Its refractive power is greater than that of nitrous oxide, but less than that of water; it is nearly equal to that of carbonic acid. The pressure of its vapour at the temperature of 50°, is equal to about 40 atmospheres.

Chlorine.

The refractive power of fluid chlorine is rather less than that of water. The pressure of its vapour at 60° is nearly equal to 4 atmospheres.

Attempts have been made to obtain hydrogen, oxygen, fluoboracic, fluosilicic, and phosphuretted hydrogen gases in the liquid state; but though all of them have been subjected to great pressure, they have as yet resisted condensation.

* Quarterly Journal of Science, vol. v. p. 74.

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The difficulty with regard to fluoboric gas consists, probably, in its affinity for sulphuric acid, which, as Dr. Davy has shown, is so great as to raise the sulphuric acid with it in vapour. The experiments will however be continued on these and other gases, in the hopes that some of them, at least, will ultimately condense.

LXXXVIII. *An Examination of the green Garnet of Sala.* *By* B. G. Bredberg.*

THE garnet of Sala, according to Haüy's crystallographical nomenclature, is *Grenat trapezoidal*, since it is bounded by 24 trapeziums, which are perceptibly striated parallel to the greater diagonal. The crystals are of a brownish yellow, sometimes of a yellowish green colour. Their surface has a resinous lustre; fracture uneven; lustre of the fracture dull; in thin splinters transparent; sometimes the crystals are semi-transparent throughout. They occur in a matrix of common limestone with crystals of calcareous spar, galena, and blende. At present this garnet is only met with in collections, since it has not been found for a long time in the mine itself. The specific gravity of a regular crystal was 3·746. Its result before the blowpipe is described in Berzelius's treatise on the application of that instrument as translated (into German) by H. Rose, p. 259.† The experiments with the blowpipe there mentioned, were undertaken with that species of garnet which is the subject of the analysis No. 2; that which was made use of for analysis No. 1 gave, on trying it before the blowpipe, a perfectly similar result. The analyses were made in the laboratory of Prof. Berzelius, where I had a favourable opportunity of acquiring the most preferable analytical method.

For the analysis No. 1 regular crystals were employed of a beautiful specimen, which, with several others, was found in the old mine by M. Pihl, captain of the mines, in the year 1780.

In No. 2, intended as a correcting analysis, crystals from the collection of Prof. Berzelius were made use of. The

* Originally published in the Trans. Roy. Acad. Stockh., but above from a translation in Schweigger and Meinecke's *Journal*, N. R., band viii. p. 11.

† In Mr. Children's valuable translation of this work into our own language the results obtained by subjecting this mineral to the agency of the blowpipe are thus described, at p. 282. "Fuses (*without addition*), with strong intumescence, into a black brilliant glass. *With borax* fuses slowly and difficultly into a glass coloured by iron. *With salt of phosphorus* decomposes slowly, and leaves a silica skeleton. The tint from iron disappears on cooling. *With soda* decomposes and intumesces, but afterwards fuses into a black brilliant globule. On platina foil exhibits traces of manganese."—Edit.

specimen

specimen belonged to such as were found at a later period in the same part of the mine, in 1800, by the mining-master M. Billow. There was no apparent ground to expect so great a difference as the analyses afterwards indicated in the composition of two specimens so similar in their appearance, their conduct before the blowpipe, and their form, and which had been found in the same mine.

Analyses.

In No. 1, the levigated powder was decomposed by concentrated muriatic acid, in which it was boiled for three days consecutively, after which the silica remained behind in gelatinous lumps. In No. 2, on the contrary, the mineral was treated with carbonate of potassa, and exposed to a red-heat in a platinum crucible. In other respects both were proceeded with in the following manner:

The acid fluid, after the separation of the silica, was precipitated by a trifling excess of caustic ammonia; the precipitate, after the lapse of some hours, was placed on a filter, and washed with boiling water, and then boiled for an hour with caustic potassa. The alkaline solution of alumina was supersaturated with muriatic acid, and the earth precipitated by carbonate of ammonia, washed and exposed to a red-heat. The oxide of iron left undissolved by the caustic lixivium, was dissolved in muriatic acid; the solution mixed with a little nitrous acid was made boiling hot, then neutralised with caustic ammonia, and precipitated with succinate of ammonia. The succinate of iron was converted to a red oxide in an open platinum crucible. The fluid obtained after the first precipitation by caustic ammonia was diluted, warmed, and precipitated with a solution of oxalate of potassa; the precipitate collected on the filter was washed, and exposed to a red-heat in a platinum crucible. In determining the quantity of lime, I tried it, for the sake of certainty, with carbonate of ammonia, and when, after two or three such trials, no alteration in weight took place, the proportion of lime was calculated from the weight of carbonate of lime thus obtained. The solutions, after the separation of the lime and iron, were put together, mixed with a few drops of muriatic acid, in order to keep the difficultly soluble oxalate of magnesia in a state of solution, and afterwards mixed, in a boiling state, with a sufficient quantity of carbonate of potassa. After evaporating them to dryness, and redissolving in boiling water, they yielded magnesia. In No. 1, a trace of manganese was indicated by this process. In No. 2, on the contrary, the earth was scarcely discoloured after ignition.

The

Fig. 1. A Common Chain SUSPENSION BRIDGE.

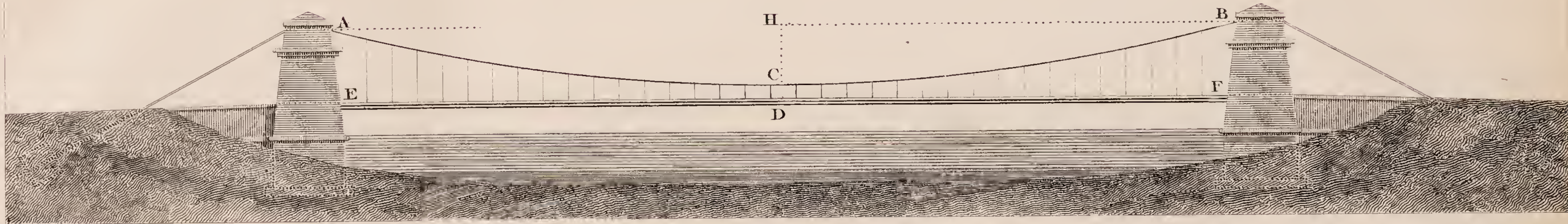
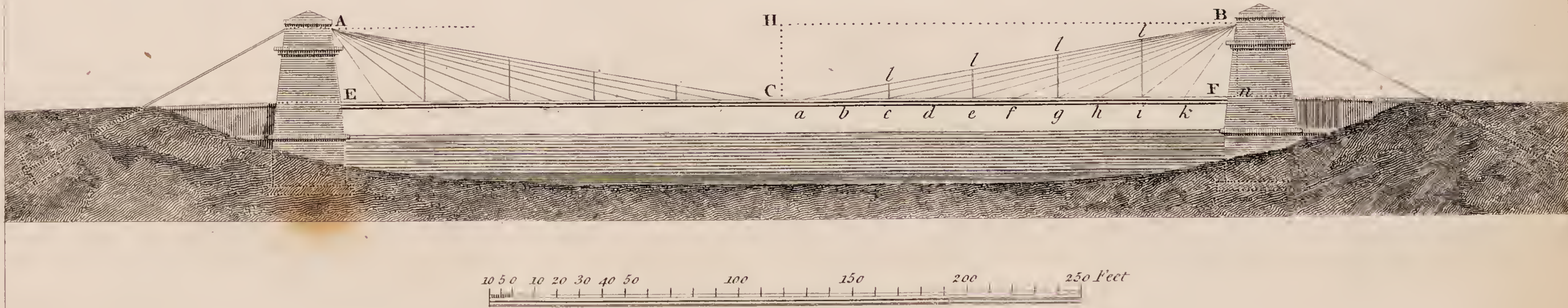


Fig. 2. A SUSPENSION BRIDGE supported by Straight Diagonal Rods.



J. Seaward del.[t]

S. Porter sculp.

The following were the results of the analyses:

	No. 1.		No. 2.
Silica	36·62	...	36·73
Alumina	7·53	...	2·78
Oxide of iron	22·18	...	25·83
Lime	13·80	...	21·79
Magnesia	1·95	...	12·44
	100·08		99·57

The amount of oxygen, calculating from these results, is as follows:

	No. 1.			No. 2.	
In the silica.........	18·42	......		18·47	
In the alumina......	3·51	10·31		1·30	9·22
In the oxide of iron	6·80			7·62	
In the lime.........	8·93	9·68		5·12	9·93.
In the magnesia...	0·75			4·81	

The excess yielded by the analysis No. 1 is probably owing to a portion of protoxide of iron which the fossil appears to have contained together with the peroxide. The calculation from the proportions of oxygen gives reason for this supposition.

The mineralogical formula of these garnets of Sala is

$$\left.\begin{matrix}C\\M\end{matrix}\right\} S + \left.\begin{matrix}A\\F\end{matrix}\right\} S.$$

LXXXIX. *Observations on Suspension Chain Bridges; with an improved Method of forming the supporting Chains or Rods: accompanied with a Drawing. By* Mr. J. Seaward.

To the Editors of the Philosophical Magazine and Journal.

Being some time back engaged in examining the Plans of a Suspension Bridge proposed to be erected in a distant part of England, I was forcibly struck with what appeared to me to be a great sacrifice of strength in the mode which is usually adopted in forming the suspending chains of such structures.

Under this impression, I was induced to offer a plan for a suspension bridge on quite a different principle; the peculiar recommendation of which is to ensure much greater strength and stability from a given quantity of materials, than what can be obtained according to the present plan. My design was shown to several scientific gentlemen, but a want of confidence, I believe, prevented it from being adopted. My views of the subject are however unchanged, and I am satisfied that,

 were

were the principle of the design clearly understood, it would meet with a favourable reception.

The number of chain bridges and piers which are now building or projected, render these structures objects of great importance: the principles of their construction therefore cannot be too fully investigated.

In the accompanying design and description which I have the pleasure of handing you, in the hope that it may be found worthy of a place in your widely circulated publication, I have endeavoured to set the merits of the two plans fairly at issue, so that their respective advantages may be fully appreciated.

It is right you should be informed that I find I cannot claim the originality of the idea, as I have learned (but not till long after I had written the accompanying description) that a bridge on nearly the same principles was projected some years back by a gentleman of the name of Anderson, and a similar one by another gentleman of the name of Loudon. The plan of the latter is accompanied by some geometrical references, but which I do not think are quite explicit, or calculated to place the merits of the question in the clearest point of view. I am yours, &c.

12 Walcot Place, Lambeth, JOHN SEAWARD.
Dec. 11, 1823.

THE first suspension bridges that were ever formed, were probably nothing more than two or three ropes or flexible chains stretched across a river from two eminences, upon which boards were placed, and thus formed a tolerably easy communication between the opposite shores: something of this kind we are informed were the bridges of the Peruvians and other primitive nations.

But when suspension bridges became objects of attention to more polished nations, the plan of forming the roadway upon the chains themselves was soon perceived to be attended with many inconveniences; a much more eligible plan was therefore early adopted, namely, the forming the roadway in a perfectly horizontal straight line, and suspending it by means of chains attached to high towers placed at the ends of the bridge. On this plan a suspension bridge has lately been built over the river Tweed: and the noble structure intended to form a communication between the opposite shores of the Menai is proposed to be executed in a similar way. Fig. 1 is an elevation of a suspension bridge of this description:—ACB represents the chain in the form of the catenary curve; the horizontal roadway EF being supported by vertical rods attached to the suspending chain.

Notwith-

Notwithstanding the great change that has been made from the primitive plan of the suspension bridge as above described, still the original form of the suspending chain has been invariably preserved: why it has been so preserved is not easy to determine, because it is quite certain that a suspension bridge can be built equally well without having the suspending chains in that particular form.

Fig. 2 is the elevation of a suspension bridge, wherein the catenary curve is not employed; the platform of the roadway being supported by straight diagonal rods attached to the tops of the two towers. A suspension bridge may be built on this plan, with the same quantity of materials, that shall possess double the strength of one formed on the common plan, as will be fully demonstrated in the course of the following observations.

It is well known that the strain or stress on the chain at the two points A and B (fig. 1.), supposing them to be at the same altitude, is as the co-secant of the angle CBH, formed by the direction of the chain, at the point of suspension, and a horizontal line BH: radius being as half the weight of the whole bridge, including the chains and all adventitious loading. Now as it is of great importance to reduce as much as possible the height of the towers, it generally happens that the aforesaid angle CBH becomes very small; and consequently its co-secant very considerable, compared with radius; which is the reason that in many cases the strain at the points of suspension is increased to three or four times the absolute weight of the whole bridge.

But what is particularly deserving of notice, is, that it is not the middle part only of the roadway and loading that has to be supported under this small disadvantageous angle CBH; but the whole roadway and loading from end to end (E to F) have to be thus supported. But if, instead of employing the catenary curve, the roadway were to be suspended by a number of straight diagonal rods aB, bB, cB (fig. 2.), and supposing each rod to support an equal portion of the load, it is plain that the stress upon the different rods will vary considerably; for although the strain upon aB, bB, &c. (where the angle aBH, bBH, &c. is small) would still be very great; yet when we come to the rods kB, iB &c., we shall find that the strain upon them would be reduced to about one quarter of what it is on aB, bB, &c., which is a most important consideration!

And again: supposing (what is not true) that the strain upon the whole of the diagonal rods at B (fig. 2.), taken together,

gether, were equal to the strain on the catenary curve at B (fig. 1.), yet as the rods *k*B, *i*B, &c. (supporting equal portions of the load) are much shorter than the others, there would from that circumstance alone arise a very considerable saving. But taking into account the diminution of the strain on the shorter rods, as also the dispensing with the vertical rods altogether, it must be quite clear to every one, on a little reflection, that the advantages in point of strength must be decidedly in favour of a bridge built on the new plan.

But as it will be much more satisfactory to illustrate what has been stated above by a practical example, I will proceed to describe, as far as is essential, a suspension bridge which has been proposed to be erected in a distant part of England on the common principle of the catenary curve: and comparing it with one recommended to be built on the new plan, with the same quantity of materials, the superiority of the latter will readily appear.

The bridge in question is proposed to be 400 feet between the points of suspension A and B (see fig. 1.); the height of the points of suspension from B to F 33 feet; the versed sine or deflexure of the curve H to C 27 feet; and from the vertex of the curve to the top of the platform C to D 6 feet; the length of the platform or roadway from E to F about 392 feet. The weight of the timber platform, the tram plates, fencing, vertical rods, the chains between the points of suspension, &c., is estimated to amount to about 150 tons; and on an extraordinary occasion,—the passing of a regiment of soldiers, or of a large drove of cattle,—it is presumed might throw an additional load of 150 tons weight on the bridge, making in the whole an aggregate weight of 300 tons to be calculated for in the strength of the supporting chains.

The chains are proposed to be formed of sixteen $1\frac{3}{4}$ in. round rods connected together in 9 feet lengths by pins and shackles, and the roadway supported by 1 in. round vertical rods.

Now a flexible chain of uniform weight suspended at two points 400 feet distant from each other, and hanging freely with a deflexure of 27 feet, will, according to writers on the catenary curve, make the angle CBH equal 15° 12′ nearly: and the length of the chain will be 404·816 feet. And supposing the whole weight to be 300 tons as above stated, the strain at each point of suspension will be equal to 572·105 tons.

The strain upon every square inch of sectional area in the sixteen chains will be equal to 14·865 tons, or 15 tons nearly.

And

	Tons.
And the quantity of metal in the chains, &c., $16 \times 404{\cdot}816$ feet $\times 1\frac{3}{4}$ in. round, $=186950$ cubic inches	22·525
Add one-third extra for the joints	7·508
Add for the suspending or vertical rods	9·500
Total	$39\frac{1}{2}$

Making in the whole $39\frac{1}{2}$ tons of chains and rods to support the roadway, and allowing a strain of 15 tons on every square inch of sectional area of the chains.

We will now suppose another bridge to be built of precisely the same dimensions as the one already described; but instead of employing the catenary curve, the roadway in this case to be supported by means of 20 straight diagonal rods as shown in fig. 11, and let the weight of the roadway, chains, adventitious load, &c., be still equal to 300 tons. Now if the platform from E to F be divided into 21 equal parts, as *ab*, *bc*, *cd*, &c., and the rods *a*B, *b*B, *c*B, &c., attached to the points *a*, *b*, *c*, &c., then will each rod bear a twentieth part of the 300 tons, or 15 tons pressing perpendicularly.

But the strain upon every one of the diagonal rods *a*B, *b*B, &c., compared with the absolute weight (15 tons) pressing perpendicularly, will be as the co-secant of the angle *a*BH, *b*BH, &c., to radius: but the co-secant of the angle *a*BH is as the hypothenuse *a*B of the right-angled triangle *an*B: therefore it is plain that the stress upon each of the diagonal rods will be directly as the length of the rod itself, radius being equal to B*n*=33 feet. And as the equal parts *ab*, *bc*, &c., are equal each to 18 feet 8 inches, and F*n* is equal to 4 feet; the length of the rods *a*B, *b*B, &c., is easily found, and consequently the strain also. Thus the length of *a*B is equal to 193·46 feet. And as rad. (33 feet) : 15 tons : : co-sec. $\angle a$BH (193·46 feet) : 87·93 tons, equal the strain on that rod. Now if we allow 7 tons only of strain upon every square inch of sectional area of the diagonal rods, we shall thereby obtain the requisite dimensions for the rod, which for *a*B will be found equal to 12·56 square inches of sectional area. And if the length of the rod be multiplied by the sectional area thus found, it will give the quantity of metal in the rod, which in *a*B is equal 29·158 cubic inches.

In the same way the length, sectional area, and quantity of metal in each of the other rods *b*B, *c*B, *d*B, &c., may be ascertained, as in the following table:

Table

Table of the Length, Sectional Area, &c., of the diagonal Rods.

			Length of Rod. Feet.	Strain. Tons.	Sectional Area in square inches, allowing 7 tons per square inch. Inches.	Cubic inches of metal in each rod. Inches.
No. 1	rod	*a*B,	193·46	87·93	12·56	29·158
2	do.	*b*B,	175·13	79·12	11·30	23·747
3	do.	*c*B,	156·82	71·28	10·18	19·157
4	do.	*d*B,	138·61	63·00	9·	14·969
5	do.	*e*B,	120·55	54·79	7·82	11·312
6	do.	*f*B,	102·75	46·70	6·67	8·224
7	do.	*g*B,	85·27	38·75	5·53	5·658
8	do.	*h*B,	68·48	31·13	4·45	3·656
9	do.	*i*B,	52·87	24·03	3·43	2·176
10	do.	*k*B,	40·03	18·19	2·60	1·249
						119·306
			And for the two ends of the bridge			2
						238·612

Equal to	28·8 tons
Add one-third extra for joints	9·6
(N. B. From there being no necessity of having the rods in short pieces, as is the case with the chains, one-third extra would not be required for the joints; but as there will be something additional for bolting the rods to the platform, the same allowance is made here.)	
Add for struts *lc*, *le*, *li*, &c. for keeping the rods straight	1·1
Total .	39½ tons

Making a total of 39½ tons of metal in the rods, being the same quantity as would be required for the chains, as already shown; but with this important difference, that while the strain upon the catenary curve would be equal to 15 tons per square inch of sectional area, upon the diagonal rods the strain would be only 7 tons per square inch. Whence it is manifest that, by adopting the new plan, a suspension bridge might be built with the same quantity of materials, and possessing more than double the strength of one built on the old plan of the catenary curve.

It is right here to observe, that the plan is not entirely free from objections: several have been made; but the only one which appears to be deserving of particular consideration is the following, viz. "Supposing it to be perfectly true that, by employing the diagonal rods in a suspension bridge, the strain is

is reduced one-half, while the load is uniformly distributed over the bridge; yet it is likely to happen that such an accumulation of weight may take place at some one point, as would produce a greater strain than could possibly occur in the catenary curve; because under such circumstances the whole weight would have to be supported by one set of rods only; whereas in the catenary curve every part of the chain would be called into action, and therefore the strain upon the latter would be proportionably much less."

In answer to this objection it is proper to remark, that the greatest accumulation of weight that could possibly occur to affect the suspending rods on the new plan, would most likely be by two heavy road waggons going over the bridge at the same time in contrary directions, and passing each other; it being presumed that, in such case, the whole weight of the two waggons would be thrown on one set of rods only.

Now the weight of two large road waggons, with their loading included (as limited by act of parliament), is 13 tons; but which may be stated at 15 tons, to compensate for any casual over-weight: and the weight of every portion of the platform *a b*, *b c*, *c d*, &c. is about $7\frac{1}{2}$ tons, making together $22\frac{1}{2}$ tons, which, under these circumstances, may be thrown on one set of the rods. But it has already been shown, that a weight of 15 tons passing perpendicularly would cause a strain of 7 tons per square inch of sectional area on the diagonal rods; of course a weight of $22\frac{1}{2}$ tons would increase the strain to $10\frac{1}{2}$ tons per square inch. Whence it would appear that circumstances might occur to throw a strain on the rods of $3\frac{1}{2}$ tons per square inch more than what it was proposed they should sustain: and thus the presumed merits of the plan would be considerably diminished.

Now admitting, for the sake of argument, that the above conclusions are just, it is but fair to remark, with respect to the catenary curve, that it is quite as likely this latter may be affected by a strain of 15 tons per square inch, as that the diagonal rods, according to the foregoing reasoning, may be affected with a strain of $10\frac{1}{2}$ tons per square inch. Therefore, allowing the full force of the objection, it is still manifest that the advantages are decidedly in favour of the new plan.

But on examining the matter a little more closely, it will be found that the objection rests upon two assumptions which are quite inadmissible. In the first place, it is assumed that the whole weight of the two waggons must necessarily be thrown upon one point of the bridge, and consequently sustained by one set of rods only. But the weight of the two waggons never can be so drawn into one point; because the hinder

hinder wheels of a waggon are generally 7 feet apart from the others; and therefore when the fore wheels of the two waggons come in a line with a set of rods, it follows that the whole weight will *not* press upon that set of rods only, but a considerable part will be thrown on the contiguous rods: and place the waggon in any other relative position, the effect produced will be nearly the same.

In the next place, it is assumed that the platform of the roadway will be made of such flexible materials, that a weight coming upon any particular point, that point must necessarily be pressed down, without communicating any motion to the adjacent parts, and thus all the weight would be thrown on one set of rods only. But this is not true of the timber platforms of suspension bridges; for it is usual to make them so very firm and stiff, that no point can be borne down without carrying a considerable length each way of the platform with it.

The platform of the proposed bridge would be constructed of longitudinal planking, three and four inches thick, spiked down to transverse joists 10 inches deep and 6 inches thick: the latter being supported by four longitudinal beams, 17 inches deep and $8\frac{1}{2}$ inches thick each, reaching from one end of the bridge to the other;—the whole being firmly bolted and secured together, and forming a strong *compages* of timber work 27 inches deep and 22 feet wide. If a portion of such a platform 37 feet long were to rest upon its two ends, and without being supported in any other part, it would admit a load of 15 tons to pass over it and not sink in the middle in any sensible degree.

It is therefore quite evident, that the whole weight of the two waggons could not bear upon one set only of the diagonal rods; on the contrary, it could be satisfactorily proved that not more than half the load could be thrown, under any circumstances, upon a single set of rods. Now half the weight of the waggons would be $7\frac{1}{2}$ tons, which added to $7\frac{1}{2}$ tons, the weight of a portion of the platform, would make 15 tons pressing perpendicularly; and this, as has been already shown, would produce a strain of 7 tons per square inch of sectional area in the diagonal rods.

It could be very easily demonstrated, that the objection, if it have any value at all, would apply with equal force against a bridge built on the principle of the catenary curve, in which it is known that all partial sinking of the road, in consequence of accumulated weight, is prevented by the stiffness of the platform, which causes the weight to be distributed over a considerable extent of the chain and vertical rods.

It

It is presumed that what has been stated is quite sufficient to show that the objection is altogether groundless. And now to conclude, with a short recapitulation of the merits of the question: It is to be noted, on the one hand, that in the suspension bridge built on the principle of the catenary curve, the weight of the platform, chains, &c. (without any loading), will throw a strain of $7\frac{1}{2}$ tons per square inch on the suspending chain; and that this strain may with great probability be increased to 15 tons: while, on the other hand, by employing the diagonal rods, the weight of the platform would produce a strain of only $3\frac{1}{2}$ tons per square inch, which strain could never exceed 7 tons per inch. In short, the strain in the new plan would, under a parity of circumstances, be always less than half what it would be in the old plan.

XC. *Derivative Analysis; being a new and more comprehensive Method of the Transformation of Functions than any hitherto discovered: extending not only to the Extraction of the Roots of Equations, but also to the Reduction of Quantities from the Multiples of Powers or Products to other equivalent Expressions, by which the Summation of any rational Series may be readily effected.* By Mr. PETER NICHOLSON.

5 Claremont-place, Judd-street.

[Concluded from p. 355.]

TRANSFORM the function $3x^2+4x-1$ into the function $3(x-0{\cdot}2)^2+B_2(x-0{\cdot}2)+C_2$ or to find an equivalent function in v, where v shall be two tenths of unity less than x, that is $x=v+0{\cdot}2$

$-0{\cdot}2$	$0 \,\vdots\, 0$	
$-0{\cdot}2$	$0 \,\vdots$	
	$4{\cdot}0$	-1
$3\times{\cdot}2={\cdot}6$	$4{\cdot}6\times{\cdot}2={\cdot}92$	$-{\cdot}08$
6	$5{\cdot}2$	

We may here observe, by the by, that since to extract the root of an equation is nothing more than to diminish that root by the whole of itself, that is, by taking away the whole of the absolute number, we have now taken away ·2 from the root of the quadratic equation $3x^2+4x-1=0$; and if we continue the same process by taking away a part, we shall at last arrive at the root, or as many true figures of the root as are found. In order to find a second figure in the root, we have only to annex a cipher to the absolute number ·08, and divide it by the preceding coefficient 52; then the number of

integers not exceeding nine is the next figure of the root, that is $80 \div 52$ gives 1 for the next figure of the root.

We must now transform the function $3v^2 + 5{\cdot}2v - {\cdot}08 = 0$ into $3(v - 0{\cdot}01)^2 + B_2(v - 0{\cdot}01) - 0{\cdot}08$ by the same process; therefore

$$\begin{array}{r|l|r}
 & 5{\cdot}20 & -{\cdot}0800 \\
\hline
3 \times {\cdot}01 = {\cdot}03 & 5{\cdot}23 \times {\cdot}01 = {\cdot}0523 & -0277 \\
{\cdot}03 & 5{\cdot}26 &
\end{array}$$

Again: $2770 \div 526$ gives 5 for the next figure of the root, therefore

$$\begin{array}{r|l|r}
 & 5{\cdot}260 & -{\cdot}027700 \\
\hline
3 \times {\cdot}005 = {\cdot}015 & 5{\cdot}275 \times {\cdot}005 = {\cdot}026375 & -{\cdot}001325 \\
{\cdot}015 & 5{\cdot}290 &
\end{array}$$

Again: divide 13250 by 5290 gives 2 for the next figure of the root, therefore

$$\begin{array}{r|l|r}
 & 5{\cdot}2900 & -{\cdot}001325000 \\
\hline
3 \times {\cdot}0002 = {\cdot}0006 & 5{\cdot}2906 \times {\cdot}0002 = {\cdot}00105812 & -{\cdot}00026688 \\
{\cdot}0006 & 5{\cdot}2912 &
\end{array}$$

and so on.

The method which is here investigated and exemplified in various applications, is not only more general, more convenient, more obvious, but also less laborious, than any other yet invented; it groups all the elementary branches of algebra in one general formula.

Scholium.

The world has been much indebted to Mr. Holdred, as being not only the first who invented a general method of extracting the roots of equations of all orders; but as being also the first to discover the best mode of abridging the labour, when a certain or given number of figures was to be found in the root.

In confirmation of this assertion, I have to state, that so early as the year 1810, which was nine years before the publication of any plan to accomplish the same object, Mr. Holdred submitted his method to me; but my engagements at that time prevented me from entering into the subject. I have never made any pretensions to the discovery of Mr. Holdred, or of any other individual; but I solemnly affirm that upon my seeing his figurate method, I discovered the non-figurate mode from a consideration of my general method of transforming functions, published in my Combinatorial Analysis in the year 1818, which was just before Mr. Holdred had communicated to me his method of extracting the roots of equations.

Upon

Upon my first examination of the figurate method, I perceived that the same process which served to transform equations by diminishing their root would also extract their roots, if each root were continually diminished by a figure at a time. It was therefore evident to me, that whether Sir Isaac Newton's method or my own were applied, the object would be accomplished by either; but as the theorem which I had already discovered was also a theorem for the summation of figurate numbers, I immediately reduced Newton's method to my own formula, and from this source alone I derived the rule which I have applied to the non-figurate method in my own publications.

This general method of transforming functions first occurred to me when writing my Introduction to the Method of Increments, published in 1817 by Davies and Dickson. In page 126 of this Introduction, my general method of transformation is hinted at; and in the same page an example of its use is given exactly in the same form as it was published in my Combinatorial Essays in 1818, the year following, and as it is now published in the Philosophical Magazine of November 1823.

When I first published the non-figurate method in my treatise on Involution and Evolution, as also in the extraction of roots in my analytical essays, I was induced, in order to abridge the operation, to omit the first column, which consisted of repetitions of the figures of the root; and also the columns of the multiples of the successive sums; but the numerous objections that were made to multiplying and adding in one line, which was rendered necessary by this method, have determined me to give the operation in full, as I had originally published it in my Method of Increments, and more particularly in my Essay on Binomial Factors, published along with the Combinatorial Analysis.

The application of my method of transformation to the extraction of roots I have considered only as an improvement on Mr. Holdred's figurate mode; and although the principle may be seen in my Introduction to the Method of Increments and in the Combinatorial Analysis, which were both published before Mr. Holdred communicated his general method of extracting the roots of equations; yet it is doubtful whether my method of transformation would ever have been applied to the extraction of the roots of equations, had I not previously seen Mr. Holdred's figurate mode*.

* I make this admission, because a similar improvement occurred to Mr. Holdred himself shortly after he had communicated the figurate method to me.

To show the application,

Let it now be required to extract the square root of the number 3856724.

Divide the number into periods of twos from right to left thus: 3,85,67,24.

Take the square root of the last figure 3, which is 1, and proceed as if it were required to diminish the root of the quadratic equation $x^2+0x-3=0$ by 1.

$$\begin{array}{r|l|l} & 0 & -3 \\ \hline 1\times1=1 & 1\times1=1 & -2 \\ 1 & 2 & \end{array}$$

20÷2 gives 10

9 however will only succeed, therefore

$$\begin{array}{r|l|l} & 20 & -285 \text{ by annexing 85 to 2} \\ \hline 1\times9=9 & 29\times9=261 & -24 \\ 9 & 38 & \end{array}$$

240÷38 gives 6, therefore

$$\begin{array}{r|l|l} & 380 & -2467 \text{ annexing 67 to 24} \\ \hline 1\times6=6 & 386\times6=2316 & -151 \\ 6 & 392 & \end{array}$$

1510÷392 gives 3

$$\begin{array}{r|l|l} & 3920 & -15124 \\ \hline 1\times3=3 & 3923\times3=11769 & -3355 \\ 3 & 3926 & \end{array}$$

and so on.

And hence the common method of extracting the square root.

$$\begin{array}{l|r} 1 & 3,85,67,24(1963 \\ 1 & 1 \\ \hline 29 & 285 \\ 9 & 261 \\ \hline 386 & \cdot2467 \\ 6 & 2316 \\ \hline 3923 & 15124 \\ & 11769 \\ \hline & \cdot3355 \end{array}$$

The preceding work is placed in such a manner as to render it obvious to inspection, and therefore description unnecessary. To apply my method of transformation to the extraction of roots, as the multipliers for each distinct figure are the same,

same, it is quite useless to place them in the operation: I will therefore give another example, and will omit them in the work.

Extract the cube root of the number 3.

Here we have only to extract the root of the cubic equation $x^3+0x^2+0x-3=0$.

Now the nearest cube root to 3 is 1; therefore

	0		0			−3
1	1	1	1	1		−2
1	2	2	3			
1	3					

Divide 20 by 3, and the quotient 6 would be the next figure; but as the work has not yet acquired a state of convergency, 6 will be found to be too much, and upon trial it will be found that no higher number than 4 will succeed; therefore

	30		300		−2000
04	34	136	432	1744	256
04	38	152	588		
04	42				

The work having now acquired a state of convergency, divide 2560 by 588, and the quotient 4 is the new figure of the root, therefore

	420		58800		−256000
004	424	1696	60496	241984	14016
004	428	1712	62208		
004	432				

Divide 140160 by 62202, and the quotient 2 is the next figure of the root; therefore

	4320			6220800	−14016000
0002	4322	8644	6229444	12458888	1557112
0002	4324	8648	6238092		
0002	4326				

And therefore the root as far as the work is extended is 1·442; and if we divide the absolute number 1557112 with four ciphers annexed by the coefficient 62208, we may have the four additional figures 2496, and thus extending the root to the eight figures 1·4422496. If we omit the first, third, fifth, &c. columns, and unite the parts of the other columns remaining, the work will stand as follows:

1	0	0	−3
	1	1	−2
	2	3	
	3		
	34	432	−256
	38	588	
	42		
	424	60496	−14016
	428	62208	
	432		
	4322	6229440	−1557112
	4324	6238092	
	4326		

And this is the same example as was published in the postscript to my Tract on Involution and Evolution, and it is the same as the operation now detailed, except that the first, third, and fifth columns are omitted.

If $\frac{A}{k} = \frac{A}{r} + \frac{B_1}{rs} + \frac{C_1}{rst} + \frac{D_1}{rstu} + \&c.$

And if $\frac{A}{kl} + \frac{B}{l} = \frac{B}{r} + \frac{B_2}{rs} + \frac{C_2}{rst} + \frac{D_2}{rstu} + \&c.$

Then will $B_2 = B(r-l) + A$, $C_2 = B_2(s-l) + D_1$, $D_2 = C_2(t-l) + C_1$ &c.

That is, the coefficient of any term n is equal to the product of the coefficient of the next preceding term $n-1$, and a certain factor plus the coefficient of the corresponding term $n-1$ of the preceding series.

This other factor is the remainder which arises by subtracting the last factor of any term of the first side of the second series from the last factor of the $(n-1)$th term of the second side.

For let
$$\begin{array}{lll} r-k=a' & r-l=a'' & r-m=a''' \\ s-k=b' & s-l=b'' & s-m=b''' \\ t-k=c' & t-l=c'' & t-m=c''' \end{array}$$

Then will
$$\begin{array}{l} k=r-a'=s-b'=t-c' \ \&c. \\ l=r-a''=s-b''=t-c'' \ \&c. \\ m=r-a'''=s-b'''=t-c''' \ \&c. \\ \&c. \end{array}$$

Divide the first side of the equation $A=A$ by k, and the second side by its equals $r-a'$, $s-b'$, $t-c'$, $u-d'$ &c. by example 1 division, page 10, and we have

$$\frac{A}{k} \cdots \cdots \cdots \cdots = \frac{A}{r} + \frac{B_1}{rs} + \frac{C_1}{rst} + \frac{D_1}{rstu} + \&c.$$

To

To each side of this equation add B, and

$$\frac{A}{k} + B \quad \ldots \quad \ldots \quad \ldots = B + \frac{A}{r} + \frac{B_1}{rs} + \frac{C_1}{rst} + \frac{D_1}{rstu} + \&c.$$

Divide the first side of this equation by l, and each term of the second side respectively by each of the equals $r-a''$, $s-b'''$, $t-c'''$, $u-d'''$ &c. by example 2 division, page 11, and

$$\frac{A}{kl} + \frac{B}{l} \ldots \quad \ldots \quad \ldots = \frac{B}{r} + \frac{B_2}{rs} + \frac{C_2}{rst} + \frac{D_2}{rstu} + \&c.$$

To each side of this equation add C, and

$$\frac{A}{kl} + \frac{B}{l} + C \quad \ldots \quad = C + \frac{B}{r} + \frac{B_2}{rs} + \frac{C_2}{rst} + \frac{D_2}{rstu} + \&c.$$

Divide the first side of this equation by m, and each step of the second side respectively by each of its equals $r-a'''$, $s-b'''$, $t-c'''$, $u-d'''$ &c., and

$$\frac{A}{klm} + \frac{B}{lm} + \frac{C}{m} \quad \ldots \quad \ldots = \frac{C}{r} + \frac{B_3}{rs} + \frac{C_3}{rst} + \frac{D_3}{rstu} + \&c.$$

Without proceeding further from the first division of the equation by k and its equals, we have

$$\left.\begin{array}{l} B_1 = a'A \\ C_1 = b'B_1 \\ D_1 = c'C_1 \end{array}\right\} \text{See the operation ex. 1 division, page 10.}$$
$$\&c.$$

From the second division of the equation by l and its equals, we have

$$\left.\begin{array}{l} B_2 = a''B + A \\ C_2 = b''B_2 + B_1 \\ D_2 = c''C_2 + C_1 \end{array}\right\} \text{See the operation ex. 2 division, page 11.}$$
$$\&c.$$

From the third division of the equation by m and its equals, we have

$$\left.\begin{array}{l} B_3 = a'''C + B \\ C_3 = b'''B_3 + B_2 \\ D_3 = c'''C_3 + C_2 \end{array}\right\} \text{See the operation ex. division.}$$
$$\&c.$$

And so on. Now by arranging these in columns, according to the orders of the derived equals, we have

$$\begin{array}{l|l|l|l} B_1 = a'\ A & C_1 = b'\ B_1 & D_1 = c'\ C_1 & \\ B_2 = a''\ B + A & C_2 = b''\ B_2 + B_1 & D_2 = c''\ C_2 + C_1 & \&c. \\ B_3 = a'''C + B & C_3 = b'''B_3 + B_2 & D_3 = c'''\ C_3 + C_2 & \\ \quad \&c. & \quad \&c. & \quad \&c. & \end{array}$$

The application of this theorem may be seen in my Combinatorial Analysis, under the article Binomial Factors. It applies to fractional infinite series in the same manner as the preceding theorem, which has been sufficiently illustrated by examples, does to whole numbers.

XCI. *On*

XCI. *An Account of a new Genus of* Narcisseæ, *allied to the Genus* Ajax *of Salisbury. By* A. H. Haworth, *Esq. F.L.S., &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

HAVING recently obtained specimens and a promise of roots of two interesting plants very nearly allied to the Genus *Ajax* of Salisbury, not as yet described in the technical language of Botany, I have given to them the name of *Diomedes*, and herewith transmit to you a correct description of them, as follows, and remain

Yours, &c.

Queen's Elm, Chelsea, Nov. 1823. A. H. Haworth.

DIOMEDES.

Character Genericus.

Corolla limbo hexapetalo-partita, tubo clavatim cylindraceo valido, coronâ mediocri poculiformi petalos semiæquanti. *Genitalia* recta. *Filamenta* subæqualia tubo semi-plusve deorsum connata. *Antheræ* exiguæ lineares erectæ.

Obs. Herba (Parkinsoni fide) e montibus Pyrenæis, habitu omnino Generis Ajacis *Salisb.*, cui locum in systemate proximum tenet. Ab Ajace differt filamentorum insertione, tubæ coronæque formâ; Genus Queltiam *Salisb.* quoque approximat, discrepante tubo, stylo, coronâque.

Specierum Characteres.

minor. 1. D. filamentis tribus apice solùm liberis, stylo coronâ breviore.

Narcissus totus pallidus oblongo calice serotinus minor. Park. Parad. 73. 4.

Descriptio. *Folia* viridia semunciam lata obtusa striatula. *Scapus* uniflorus, florendi tempore pedunculum superans. *Germen* post anthesin compresso-ovale. *Tubus* 6-7-linearis subcompresso-cylindraceus, supernè incrassatus sive latior, e viridi-lutescens. *Corolla* nutans, laciniis 6 subæqualibus elliptico-ovatis albis, basi distinctis luteis, stellatim expansis. *Corona* cylindrica poculiformis parum angulata laciniarum semilongitudine plusque, intensè lutea, ore recto subplicatulatim crenulato. *Filamenta* inæqualia gracilia lutescentia, supernè alba, tria tubo altè connata et libera solùm longitudine antherarum: tria dimidiatim libera, prioribus parum breviora. *Antheræ* tubum paulo superantes

rantes lineares erectæ exiguæ albæ, quasi imbecilles et forte sine polline. *Stylus* albus strictus antheras 3-4 lineas superans, at coronâ humilior, *stigmate* obsolete trilobo. *Germen* triloculare embryonibus pluribus.

Floret—Apr. medio. H. ♃.

major. 2. D. corollæ laciniis oris reflexis, filamentis plus quam semiliberis, stylo coronam æquante.

Narcissus albus oblongo calice luteo serotinus major. Park. Parad. 73. 3.

Descriptio. Priore in omnibus triplo major at similis, scapo minus striatulo læviore. *Spatha* uniflora. *Corollæ* laciniæ speciosæ incurvo-expansæ, ad oras altissimè reflectentes, basi imbricantes, tubum cum germine æquantes, vel superantes, coronâque tertiâ parte longiores. *Corona* lutea, at pallidior quam priore, ore magis plicatim crenulato. *Filamenta* æqualia tubum longè superantia, sed humiliora quam corona; tria tubo infernè connata, at supernè plus quam semilibera; tria alia aliquantulum altiùs tubo connexa. *Antheræ* erectæ, externè parum curvatulæ, colore subaurantiaco, polline magis conspicuo quam in priore, sed non abundante. *Stylus* prioris, at major, coronæ longitudine.

Floret fine Aprilis. H. ♃.

XCII. *On the Law according to which the Electro-Magnetic Power of the Connecting Wire of the Voltaic Pile is augmented by Schweigger's Multiplier.* *By* L. F. Kaemtz, *Phil. Doct., of Halle.**

1. IMMEDIATELY after Oersted's discovery had become known, the idea occurred to Professor Schweigger of increasing the electro-magnetic power of the voltaic pile by winding the connecting wire around the compass; he showed at the time, in his lectures, some experiments, with intent to examine in what degree the electro-magnetic power would be augmented by each additional convolution of the wire around the compass. The experiments, however, which were made here soon after the invention of the multiplier, were unsuccessful as to the discovery of a determinate law for this increase: (see Schrader *de Electro-magnetismo*, § 2. Schweigger's Journal, N.R. bd. i. p. 6.) I considered, therefore, that it would not be superfluous to ascertain this law by more exact experiments.

2. Before I proceed, however, to the description of the experiments

* From Schweigger and Meinecke's *Nues Journal*, band viii. p. 100.

periments themselves, I will develop a few formulæ by which the amount of the electro-magnetic power may be found from the given angles of attraction or repulsion of the magnetic needle.

M may therefore denote the power of the terrestrial magnetism;

m the magnetic power of the needle, whose length is $= 1$.

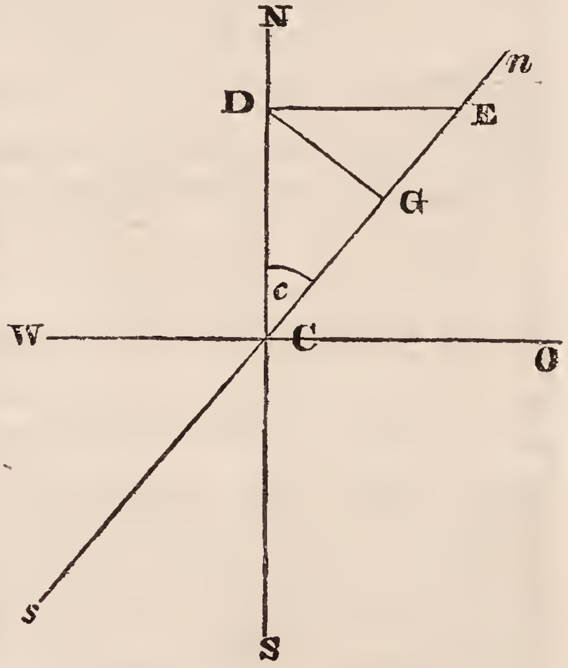
Now if the dipping needle is brought round an angle c out of the magnetic meridian, then the terrestrial magnetism strives to bring the needle into the meridian again, and with a power too which is equal to

$$M\, m.\ \sin.\ c.$$

(Compare Hansteen on Terrestrial Magnetism, part i. p. 130. Biot *Précis de Physique*, tom. ii. p. 26. edit. 2d.)

The magnetic power of the connecting wire of the electrical apparatus now acts on the needle likewise. If it be required to calculate the amount of the magnetic power of the electrical apparatus, from the angle of repulsion or attraction, where both powers (the terrestrial magnetism and the electro-magnetism) are in equilibrium, there are two cases to be distinguished: namely, the connecting wire either passes through the magnetic meridian, or forms an angle with it.

a) If the electrical stream passes through the magnetic meridian below the needle from south to north, and above it from north to south; thus does it pass in SN; then it has on the western side a southerly, and on the eastern side a northerly polarity. The north pole of the needle (*pole austral* of the French) is driven towards the east, and the needle remains stationary in *ns*.



Now E may denote the magnetic power of the connecting wire: this acts in a direction perpendicular to the axis of the wire, towards DE. Therefore we may at the same time take for granted, that DE is proportional to the magnetic power. We therefore change DE into DG and GE, in which case DG is perpendicular to *ns*. Now the relation is,

$$DE : DG = 1 : \cos EDG, \text{ that is,}$$
$$E : DG = 1 : \cos c \text{ is consequently}$$
$$DG = E \cos c.$$

The needle reacts against this power with the power m; the electro-

electro-magnetic power acts at the same time inversely as the distance (comp. Biot *Précis de Physique*, tom. ii. p. 122. Hansteen in Gilbert's Annals, bd. lxx. p. 175. Schmidt *ibid.* p. 248), therefore inversely as DE. But DE = sin. c. is for the length of the needle, which I have fixed above as $=1$. The aggregate power, with which the electro-magnetism and the magnetism of the needle act upon each other is therefore

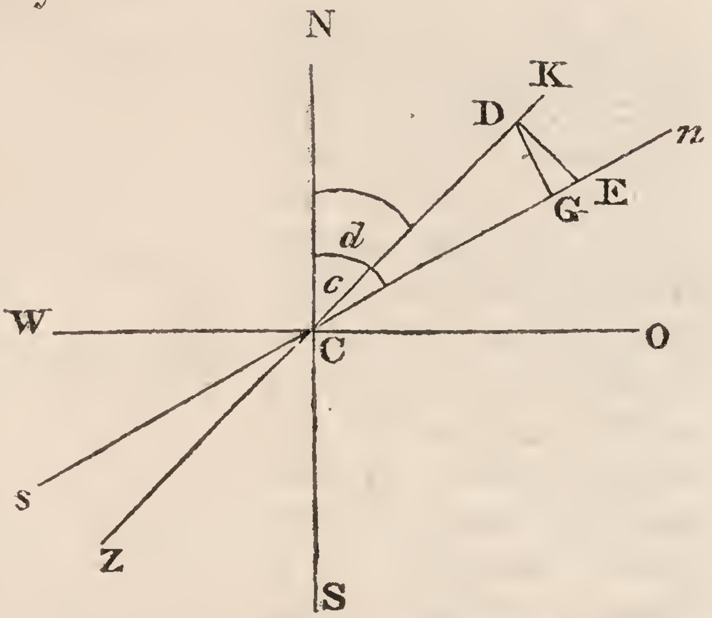
$$= \mathrm{E}m \frac{\cos. c.}{\sin. c.} = \mathrm{E}m. \cot. c.$$

Now if the needle is stationary in ns, then the electro-magnetism and the terrestrial magnetism balance each other; it is therefore

$$\mathrm{M}m \sin. c = \mathrm{E}m. \cot. c, \text{ and from this}$$

$$\mathrm{E} = \frac{\sin. c.}{\cot. c.} \mathrm{M} = \sin. c. \operatorname{tang.} c. \mathrm{M}. \qquad \text{(A)}$$

The same equation is likewise applicable, when the electric stream passes from N. to S., only that the repulsion of the north pole is then a westerly one.



b) When the connecting wire does not pass through the magnetic meridian, but makes an angle with it NCK $= d$. Here two cases are to be distinguished.

α) Let the connecting wire intersect the magnetic meridian in such a manner that the north pole of the wire and the south pole of the needle are opposite to each other. Here the magnetism of the connecting wire acts so strongly upon the needle, that the north pole of the latter is at first drawn as far as to the wire, then passes on below it, and is next repelled again on the other side, so that the needle remains at rest in ns, where $\measuredangle$ NC$n = c$ is put. If one again changes here DE = E into DG and GE, then is DG = E. cos. KCG = E. cos. $(c-d)$.

The aggregate power with which the needle and the connecting wire react on each other is therefore

$$= \mathrm{E}m \frac{\cos.(c-d)}{\sin.(c-d)} = \mathrm{E}m. \cot. (c-d), \text{ consequently}$$

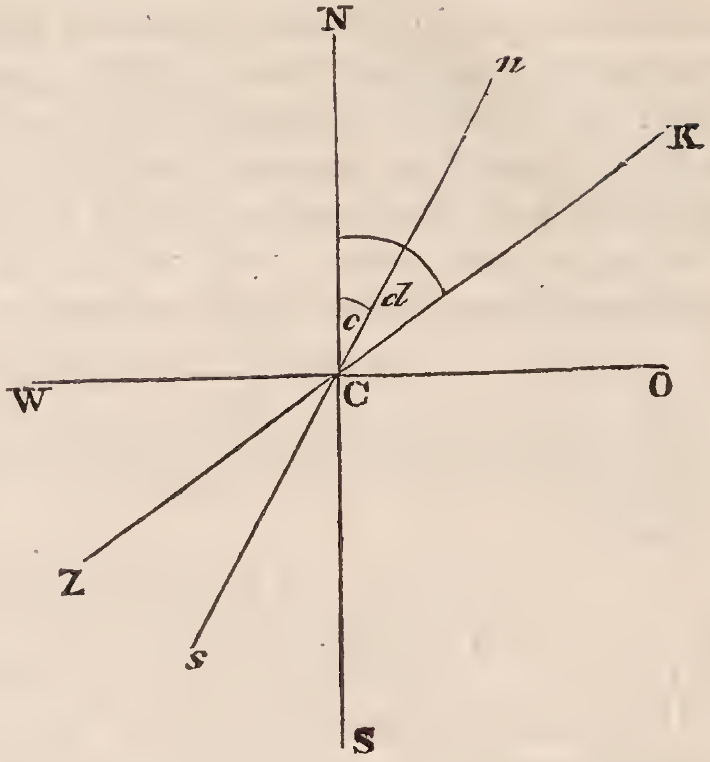
$$\mathrm{M}m \sin. c = \mathrm{E}m. \cot. (c-d), \text{ therefore}$$

$$\mathrm{E} = \sin. c. \operatorname{tang.} (c-d) \mathrm{M}. \qquad \text{(B)}$$

But

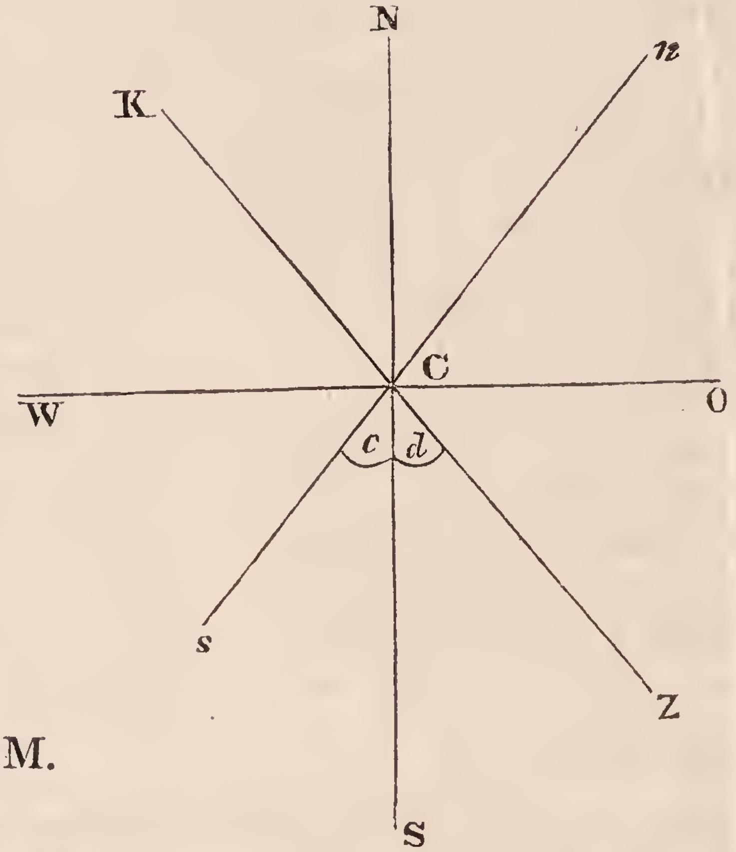
But the magnetic power of the connecting wire may likewise be of such amount only, that the needle remains stationary between it and the magnetic meridian, and therefore in *n s*. In this case we find, in a similar manner,

$$E = \sin. c. \text{tang.} (d-c) M. \quad \text{(C)}$$



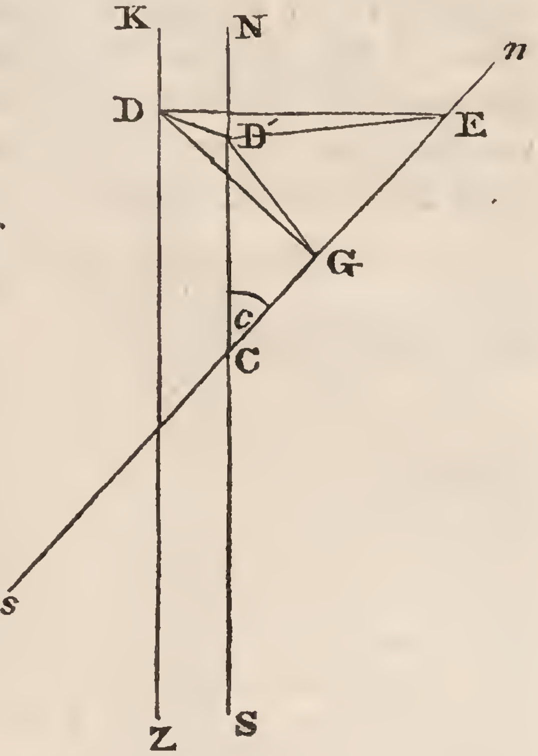
β) The connecting wire intersects the magnetic meridian in such a manner that its north pole is opposite the north pole of the needle, in the direction KZ; therefore, where there is in KZ, on the right a north, and on the left a south pole. In this case the needle is impelled towards *n s*. Here we find in the same manner as above,

$$E = \sin. c. \text{ tang. } (c+d) \text{ M.} \quad \text{(D)}$$



3) The equations hitherto developed however are not quite exact, as it was taken for granted, that the conneting wire and the needle were lying in *one* plane. If, however, the needle be very long, and the distance of the wire from it very trifling, they may always be applied, particularly on this account, that the error which is committed by neglecting this distance, is generally committed in the comparison of electro-magnetic powers, and is therefore less striking. The more exact equations however, which certainly are not so simple as the

the above, may be very easily developed in the following manner. I have here the case in view, in which the connecting wire passes through the magnetic meridian. Let KZ be the wire, NS the direction of the magnetic meridian, and the original situation of the needle, which now stands in $n\,s$ in equilibrium with the terrestrial magnetism, and moves in the plane D′EG. Now let DD′ be the distance of the wire from the plane, in which the needle moves, which distance we will state $=$X. Now DE is $=$E; if we divide this power into DG and EG, then we have



$$\text{DG}=\text{E. cos. EDG}=\text{E. cos. ED}'\text{G}=\text{E. cos. } c.$$

Now the electro-magnetic power acts inversely as the distance DE. But it is

$$\text{DE}=\sqrt{(\text{DD}'^2+\text{D}'\text{E}^2)}=\sqrt{(x^2+\sin.^2 c)}.$$

The needle and the electro-magnetism act on each other, therefore, with the power

$$\frac{\text{E}m\cos.\,c}{\sqrt{(x^2+\sin.^2 c)}},$$

And because

$$\text{M}m\sin.\,c=\frac{\text{E}m\cos.\,c}{\sqrt{(x^2+\sin.^2 c)}},$$

Then is $\text{E}=\frac{\sin.\,c}{\cos.\,c}\sqrt{(x^2+\sin.^2 c)}\,\text{M}.$ (A′)

The equations B C D change in the same manner into the following:

$$\text{E}=\frac{\sin.\,c}{\cos.(c-d)}\sqrt{\left(x^3+\sin.^2(c-d)\right)}.\text{M} \quad (\text{B}')$$

$$\text{E}=\frac{\sin.\,c}{\cos.(d-c)}\sqrt{\left(x^2+\sin.^2(d-c)\right)}.\text{M} \quad (\text{C}')$$

$$\text{E}=\frac{\sin.\,c}{\cos.\,(c+d)}\sqrt{\left(x^2+\sin.^2(c+d)\right)}.\text{M} \quad (\text{D}')$$

4) The distance of the connecting wire from the horizontal plane, in which the needle moves, can be very easily measured in a mechanical manner; every one may as easily perceive, however, that this method promises little precision. The equations themselves fortunately offer the means of determin-

ing

ing the value of x. It is clear, namely, that E must be equipersistent for the same electromotor, and for the same fluid. Now if the angle, which the connecting wire forms with the magnetic meridian, is at one time d, at another d', in the same manner the angle of repulsion at one time c, at another c'; then in the first case:

$$E = \frac{\sin. c}{\cos. (c-d)} \sqrt{\left(x^2 + \sin.^2 (c-d)\right)} M;$$

and in the second case

$$E = \frac{\sin. c'}{\cos. (c'-d')} \sqrt{\left(x^2 + \sin.^2 (c'-d')\right)} M;$$

therefore

$$\frac{\sin. c}{\cos.(c-d)} \sqrt{\left(x^2 + \sin.^2 (c-d)\right)}$$
$$= \frac{\sin. c'}{\cos.(c'-d')} \sqrt{\left(x^2 + \sin.^2 (c'-d')\right)}$$
$$\text{or, } \frac{\sin.^2 c}{\cos.^2 (c-d)} \left(x^2 + \sin.^2 (c-d)\right)$$
$$= \frac{\sin.^2 c'}{\cos.^2 (c'-d')} \left(x^2 + \sin.^2 (c'-d')\right).$$

Whence

$$\left(\frac{\sin.^2 c}{\cos.^2 (c-d)} - \frac{\sin.^2 c'}{\cos.^2 (c'-d')}\right) x^2.$$
$$= \sin.^2 c' \tan g.^2 (c'-d') - \sin.^2 c \tan g.^2 (c-d);$$

and therefore

$$x^2 = \frac{\dfrac{\left\{\sin.^2 c'. \tan g.^2 (c'-d') - \sin.^2 c \tan g.^2 (c-d)\right\}}{\cos.^2 (c-d). \cos.^2 (c'-d')}}{\sin.^2 c. \cos.^2 (c'-d') - \sin.^2 c'. \cos.^2 (c-d)}.$$

Now, in order to determine this value of x in my experiments, I gave various values to the angle d, and observed the corresponding angle c. My experiments were the following, and were made with two electromotors.

Angle d	$-20°$	$-10°$	$0°$	$+10°$	$+20°$	
Angle c	$24° 55'$	$22° 12'$	$18°$	$14° 38'$	$11° 33'$	A
Angle c	$32° 50'$	$27° 55'$	$22° 43'$	$17° 53'$		B

If the equations for the angles in A be calculated first, and the equation for $d = -20$ placed in a series like the others, and the same be done with the angles in B, then we obtain,

$$\left.\begin{aligned} 0{\cdot}17881x^2 + 0{\cdot}0013134 &= 0{\cdot}14943\,x^2 + 0{\cdot}0066736 \\ 0{\cdot}17881x^2 + 0{\cdot}0013134 &= 0{\cdot}10557\,x^2 + 0{\cdot}0100813 \\ 0{\cdot}17881x^2 + 0{\cdot}0013134 &= 0{\cdot}077243x^2 + 0{\cdot}013419 \\ 0{\cdot}17881x^2 + 0{\cdot}0013134 &= 0{\cdot}055203x^2 + 0{\cdot}015114 \end{aligned}\right\} \text{from A.}$$

$$\left.\begin{aligned} 0{\cdot}30219x^2 + 0{\cdot}018769 &= 0{\cdot}24214\,x^2 + 0{\cdot}022915 \\ 0{\cdot}30219x^2 + 0{\cdot}018769 &= 0{\cdot}17527\,x^2 + 0{\cdot}026138 \\ 0{\cdot}30219x^2 + 0{\cdot}018769 &= 0{\cdot}12070\,x^2 + 0{\cdot}026398 \end{aligned}\right\} \text{from B.}$$

Adding

Adding these equations together, there results

$$1{\cdot}62181x^2+0{\cdot}0615606=0{\cdot}925556x^2+0{\cdot}1207389,$$

Also
$$0{\cdot}696264x^2=0{\cdot}0591783$$
$$x^2=0{\cdot}084905$$

5. Setting out from these principles, I made several series of experiments, in order to develop the law of the relation of the magnetic power of the connecting wire in Prof. Schweigger's multiplier to the number of convolutions it was made to take. For this purpose I made use of a magnetic needle six inches in length, made by M. Kraft an instrument-maker of this town. Glass tubes had been applied to the compass, at two opposite points, through which the wire was introduced. The limb was divided into half degrees, and I could very well estimate small fractions of a degree, by means of a lens. The compass stood upon a vertical pillar, revolving on its axis, at the foot of which was placed a graduated disk three inches in diameter. In this manner I could put the connecting wire into each azimuth, and vary the angle d as I pleased. I could also use the same needle as a dipping needle; I confined myself, however, to experiments with the variation needle.

The electromotor I employed was a simple alternation on Prof. Schweigger's construction (Gehlen's *Journal*, bd. vii. taf. 5. fig. 18: Schweigger and Meinecke's *Journal*, N. R. bd. i. p. 7); the strip of zinc being about eight inches long and four wide, and that of copper consequently double that size. The fluid conductor was a solution of muriate of ammonia in spring water, to which was added about 0·01 of concentrated sulphuric acid. For connecting wire I made use of copper harpsichord wire, covered with silk thread, and connected with the electromotor by finer wire (No. 14).

To the above I have to add the following observation: Several authors complain, that the results obtained by the electro-magnetic experiments can never be relied upon, *because this power rapidly decreases in a short time.* The remark may be true, but I maintain that this source of error may be entirely avoided. It appears to depend, principally, upon the construction of the electrical apparatus. If a voltaic pile be made use of, the diminution of power takes place pretty quickly; it is much slower with the apparatus consisting of a copper vessel in which a plate of zinc is placed; and it decreases slower still with the *couronne des tasses.* If the apparatus just described be employed, however, the diminution of power takes place very slowly; but the precaution is to be taken of first bringing the metals into contact with the conducting wire, and then immersing the electromotor in the fluid.

fluid. In this case, as I have convinced myself by experiments made for the purpose, the diminution of intensity may be neglected at the commencement. It is also convenient, that the electromotor be always immersed in the acid in an equable manner, not quicker at one time than at another.

The diminution of intensity appears to have some relation likewise to the region of the globe in which the electromotor is situated. This however is merely a supposition, to which I have been led by experiment; I will not venture to maintain that it is an absolute fact.

I have also to observe, that the wire had always an equal length in my experiments, which is in all cases important, since the length of the connecting wire greatly weakens the electro-magnetic power. The fluid was always of an equal temperature; for the greatest difference of temperature, which was observed, did not amount to more than 2° R., and I can therefore take it for granted, that the temperature had been equal.

6. In this manner I found the following angles for every convolution of the wire around the compass.

Angle *d*.	1 Convolution.	2 Convolutions.	3 Convolutions.	4 Convolutions.	5 Convolutions.	6 Convolutions.	26 Convolutions.
0 °	15° 7′	22° 5′	28° 30′	30° 55′	38° 12′	41° 56′	70° 20′
−20	23 58	33 47	40 52	44 57	46 18	52 12	86 10
−40	7 39(*)	13 54(*)	55 16	60 6	63 15	67 30	109 38
−60		8 5(*)	66 12	76 27	80 25	86 50	130 10
−80			4 20(*)	5 6(*)	8 48(*)	13 30(*)	164 11
−90	0	0	0	0	0	0	180
+20	9 33	14 54	19 12	21 52	27 15	30 5	50 16
+40	5 4	9 10	12 43	13 51	16 23	19 36	36 30
+60		5 8	7 5	7 58	10 2	11 30	21 12
+80			2 25	3 6	3 30	3 45	7
+90	0	0	0	0	0	0	0

This table contains, in the first vertical column, the values for the angle above denoted by d, i. e. for the angle which was made by the connecting wire with the magnetic meridian. The negative values of it indicate that in this case an attraction took place while the needle was repelled at the positive pole. The succeeding columns contain the angle c round which the needle was driven out of the meridian; the angles marked with an asterisk indicate, that the angle $d-c$, and not the angle $c-d$, is to be taken, and the equation must be applied.

All the angles are at least from ten observations, and I very seldom took that point at which the needle remained stationary; but I usually observed several arcs succeeding each other,

between

between which the needle oscillated, and took the mean of them.

7. If we now calculate the intensities of the magnetic power of the connecting wire, we find, that if the power of one single convolution is made $=1$, the power of n windings is $=n$, and that this apparatus may be more appropriately called a multiplier than a condensator. The values found are as follows:

Number of Convolutions.	Coefficient of M.	Relative Proportion to one Convolution.	
		calculated.	observed.
1	0·101749	1	1
2	0·214004	2	2·103
3	0·310509	3	3·052
4	0·408097	4	4·011
5	0·492592	5	4·841
6	0·605523	6	5·951
26	2·498289	26	24·652

That the law just now established does not exactly apply for 26 convolutions, cannot in any respect be considered as an instance of inaccuracy in it, but is probably an error in the observation of a convolution.

The law is confirmed moreover in the following manner: If the connecting wire intersect the magnetic meridian under an angle of 90°, then we know that the needle is turned back, if the electric stream passes from W. to E. After I had previously calculated the intensities for one or two convolutions, I then calculated the number of convolutions for this case. Then we have $E=M$, consequently the number of the requisite convolutions $= \frac{1}{0 \cdot 1017} = 9 \cdot 7$. I took therefore at first 9 convolutions, then 10; in both cases the needle remained stationary; but at 11, it immediately turned back very quickly.

It results at the same time from the above, that if the connecting wire pass through the magnetic meridian, the needle can never be repelled at 90°; for in this case, according to the equation (A′), we shall have

$$E = \text{tang. } 90° \sqrt{(x^2+1)}\, M;$$

but as tang. $90° = \infty$, then the magnetic power of the connecting wire should be infinitely great, therefore the magnetic power of the earth ought to be $=0$.

XCIII. *Suggestions for rendering the Labours of Foreign Astronomers available in Great Britain.*

To the Editors of the Philosophical Magazine and Journal.

ALL persons who are fond of astronomical pursuits must be grateful to those correspondents who occasionally convey important intelligence by means of the Philosophical Magazine. The article lxxxvii, in the No. for last month, contains much information which probably would have been confined to a few men of science in the metropolis, had not the liberality of the writer communicated it to the pages of your Magazine. The activity of the foreign astronomers appears very remarkable; but from their works being principally written in German, and from the difficulty of procuring them, the labours of these philosophers remain in a great degree unheard of by many. It would be a subject worthy the attention of the Astronomical Society, to request their Secretaries to communicate any circumstances which may be curious or useful, received from the continent, for the information of the distant members. I beg leave to suggest to this Society, how desirable it would prove, if the occultations which are given in the Philosophical Magazine, from Inghirami, for 1824, could be published, as observed by those experienced astronomers who have accurately verified the position of their observations. But it would be still more gratifying if the time of some of these occultations as seen at Greenwich could be made known; but this is perhaps too much to ask. It would also be extremely advantageous to know the culmination of the moon's preceding limb, in sidereal time, as seen at Greenwich during the early part of the moon, for the purpose of comparing longitudes by means of stars near her course, as mentioned in page 392 of the Philosophical Magazine of last month. The theory of this operation is described at page 854 of Woodhouse's Astronomy, &c.; and it would oblige many observers if the formulæ for the correction of the process, which are said (page 392) to have been prepared by Nicolai, Bessel, &c., could be communicated through the pages of the Philosophical Magazine, as the works of these great men are in the hands of few in this country. Dr. Brinkley's formula is generally used; but still, without simultaneous cooperation, both occultations and lunar transits become little more than amusing sights; for, unless the observed time at Greenwich were accurately known, much uncertainty must remain; as the *calculated* time, from the imperfections of lunar tables, could not be implicitly relied on, to seconds, in deducing longitudes at distant places. It must be remembered also,

also, that unless the distances were within certain limits, the preceding limb of the moon would not be so proper as the centre, on account of the change in the moon's diameter. But this would not be necessary for experiments within the shores of our island. Should any gentlemen, in various parts of the country, be desirous of making observations of this kind, the writer would with pleasure prepare a table of the moon's place corrected, and a few selected stars preceding and following of the same declination, as near as possible, for the early age of the moon, during the beginning of the next year, and transmit it to the Philosophical Magazine. The result of such observations would ascertain to what degree of accuracy this method would answer the purpose intended. But the observed time at Greenwich is the great desideratum, as a guide to all observers, in a series of experiments of this nature. M.

Dec. 9, 1823.

XCIV. *Notices respecting New Books.*

A SECOND edition of Mr. Tredgold's Essay on the Strength of Cast Iron is just published; with considerable additions. These additions consist in popular illustrations and examples of the rules; a great variety of new experiments on cast iron, from whence the relation between the quality and the appearance of the fracture has been ascertained, and the qualities of iron furnished by different iron works. A new section has been added on the strength of malleable iron and other metals, with many new experiments on the strength of wrought iron, gun-metal, brass, steel, &c. Several useful tables have been added, and the extent of the table of the properties of materials nearly doubled.

Mr. J. E. Gray has in the press, The Elements of Zoology; containing, besides an Outline of Comparative Anatomy and Physiology, and a Natural Disposition of the Animal Kingdom, with an analytical Table of the Genera, an Explanation of all the Terms used in the Science, illustrated by numerous Engravings. This work will be upon the principles proposed by W. S. MacLeay, Esq., and the modern continental naturalists.

Monographia Tenthredinetarum* synonimia extricata. Auctore Am. le Peletier de Saint-Fargeau, Societatis Parisiensis Historiæ Naturalis Membro. Paris, apud Levrault, 1823.

* The appearance of this work may interest the young Entomologist, whose inquiry relative to British Tenthredos is noticed at pp. 155 and 316.

Mr.

Mr. De la Beche will shortly publish a Selection of the Geological Memoirs contained in the Annales des Mines; together with a Synoptical Table of Equivalent Formations, and M. Brongniart's Table of the Classification of Mixed Rocks, in 1 vol. 8vo.

Messrs. J. D. C. and C. E. Sowerby are about to publish a Descriptive Catalogue of the Zoological part of their late Father's Museum; in five parts, each containing one of the great classes of animals. The first part, including *Mollusca* or their *Shells*, will commence in the spring, and will form, when complete, a systematic arrangement of all the known recent and fossil species. It will be illustrated by numerous coloured engravings.

XCV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

On Monday, December 1, (St. Andrew's Day having fallen on a Sunday,) the Fellows of the Royal Society held their Anniversary at Somerset House. At 12 o'clock, when Sir Humphry Davy took the chair, there was a numerous attendance of the Fellows. The learned President began the business of the day by reading the list of the newly admitted and deceased Members, and on the last occasion paid a tribute of respect to the memories of Dr. Jenner, Dr. Hutton, Dr. Baillie, and Col. Lambton, by describing the characteristic labours, virtues, and talents of these eminent men. He then proceeded to state the award by the Council of the Copley Medal to Mr. Pond, the Astronomer Royal, for his various communications published in the Transactions of the Royal Society.

In a discourse, which was received with the most profound attention by the Fellows, the President gave a view of the important labours which had been carried on in the Royal Observatory since its foundation by Charles II., and which had led to the most important discoveries made in modern times in astronomical science. He entered into an animated panegyric of Flamsteed, Halley, Bradley and Maskelyne, and spoke of the glory arising to this country from the immediate or ultimate results of these researches, which, illustrated by, and throwing light upon, the mathematical laws of the motions of the heavenly bodies developed by our own illustrious Newton and his school, have given to us the true knowledge of the system of the universe. He spoke of the benefits which had been conferred,

conferred, by the observations made at Greenwich, on navigation, and our maritime interests, repaying a hundred fold the liberal expenditure of Government on this great national establishment. In speaking of the labours of Mr. Pond, he mentioned that the two most important points of research to which he had directed his attention, were the question of the parallax of the fixed stars, and observations which seem to show a considerable apparent southern motion of many of the principal fixed stars. Mr. Pond thinks there is no evidence of a sensible parallax. Dr. Brinkley, on the contrary, is of opinion that this parallax distinctly exists. The Council of the Royal Society, said the learned President, do not mean in any manner by their award of the medal to express an opinion on this subject*; for when two such observers differ, the question cannot be considered as settled: and he paid the highest compliments to the profound mathematical knowledge, acuteness and accuracy of research, and extent of view, of Dr. Brinkley; and between his observations and those of the Astronomer Royal, the problem of parallax was now, he said, reduced within very narrow limits; but perhaps more perfect instruments and observations will be required for its complete solution. On the supposed southern declinations of the fixed stars it is impossible, said the learned President, to form at present any correct judgement—such an important result could only be established by new observations carried on for a great length of time, and confirmed by the experience of the best astronomers in different countries.—He desired Mr. Pond to consider the medal as a mark of the respect of the Society for the zeal and ardour with which he had pursued astronomy, and as showing their confidence in the general accuracy of his observations. He likewise requested him to regard it as a pledge, that future important labours were expected from him. He exhorted him to emulate the fame of his great predecessors, and to endeavour to transmit his name to posterity by similar monuments of utility and glory.

The Society then proceeded to the election of a Council and Officers for the ensuing year, when the following gentlemen were chosen:

Of the Old Council.—The Right Hon. Sir H. Davy, Bart.;

* We are happy to find this sentiment thus publicly announced from the chair, as it at once shows the judgement and impartiality of the President; and removes every idea that the Council of the Royal Society have, by their vote, declared any opinion as to the existing discussions relative to the *parallax* of the fixed stars, or as to the recent assertions of the Astronomer Royal relative to their *southern motion*.—Edit.

W. T.

W. T. Brande, Esq.; the Lord Bishop of Carlisle; Taylor Combe, Esq.; J. W. Croker, Esq.; Davies Gilbert, Esq.; Charles Hatchett, Esq.; Sir Everard Home, Bart.; John Pond, Esq.; W. H. Wollaston, M.D.; Thomas Young, M.D.

Of the New Council.—William Allen, Esq.; Major Thomas Colby; James Ivory, Esq.; Sir James M'Gregor, Bart.; William Marsden, Esq.; W. G. Maton, M.D.; His Grace the Duke of Norfolk; Edward Rudge, Esq.; William Sotheby, Esq.; Henry Warburton, Esq.

President.—The Right Hon. Sir H. Davy, Bart.

Treasurer.—Davies Gilbert, Esq.

Secretaries.—W. T. Brande and Taylor Combe, Esqrs.

Foreign Secretary.—Thomas Young, M.D.

Dec. 11.—A paper was read, On the Nature of the Acid and Saline Matter usually existing in the Stomachs of Animals, by William Prout, M.D. F.R.S.; and the reading was commenced of An Inquiry respecting the supposed Heating Effect beyond the red End of the Spectrum, by B. Powell, M.A., of Oriel College, Oxford.

John Bayley and George Townley, Esqrs., were admitted Fellows of the Society; and MM. Fourier and Vauquelin were elected Foreign Members.

Dec. 18.—A communication was read On the North Polar Distances of the principal Fixed Stars, by J. Brinkley, D.D. P.R.I.A. F.R.S. This paper, as far as we could judge of it, appeared to be a direct attack on Mr. Pond's recent doctrine relative to a *southern motion* of the fixed stars. The learned author adduces observations of Bradley in 1728, of Cassini in France in 1740, of Dr. Maskelyne at Schehallien, of Piazzi at Palermo, of Mudge in England in 1802, and of Lambton in Hindostan in 1805; and endeavours to show that the southern motion belongs entirely to the Greenwich instruments and observations. The author also complains that his catalogues of 1813 and 1823 are misrepresented by Mr. Pond, in his papers, and that they are even altered from their original form; for Mr. Pond has diminished the quantities in Dr. Brinkley's catalogue, by applying Bradley's refraction, whilst M. Bessel's are left just as they were; and he is thus enabled to place his own as a mean between them. Dr. Brinkley has subjoined various tables to his paper as confirmatory of the points here insisted on; and the public await with much impatience, their publication, since every thing which comes from so distinguished an astronomer cannot fail to be interesting and important.

A paper was also begun, On the Figure requisite to maintain the Equilibrium of a homogeneous Fluid Mass that revolves upon an Axis, by James Ivory, Esq., M.A. F.R.S.

LIN-

LINNÆAN SOCIETY.

Dec. 2.—A communication by Mr. David Don, Librarian of the Society, was read, entitled Descriptions of Nine new Species of the Genus *Carex*, Natives of the Himalaya Alps in Upper Nepal.

In forming the character of the species, Mr. Don professes to have followed as a model the learned Bishop of Carlisle's Monograph of the British Species of this Genus, Linn. Trans. vol. xi. The species described are:—

1. C. *nubigena*, digyna; spiculis subnovenis ovatis confertis, arillis ovatis striatis rostratis bifidis margine denticulato-scabris, glumis ovatis acuminatis, culmo striato nudo infernè tereti, foliis involutis.—2. C. *foliosa*, digyna; spicâ elongatâ, spiculis ovato-oblongis adpressis; inferioribus subremotis, arillis ellipticis rostratis bifidis margine lævibus, glumis ovatis aristatis, culmo acute triquetro scabro, foliis planis.—3. C. *flexilis*, digyna; vaginis elongatis pedunculo brevioribus, spicis filiformibus cernuis apice masculis, glumis ellipticis acutis, arillis ovatis striatis pilosis rostratis.—4. C. *macrolepis*, digyna; vaginis elongatis pedunculo brevioribus, spicis strictis cylindraceis apice masculis, glumis lanceolatis longicuspidatis, arillis ovatis rostratis scaberrimis costatis apice bipartitis. —5. C. *longipes*, digyna; vaginis elongatis pedunculo 4-plo brevioribus, spicis cylindraceis erectis apice masculis, glumis ellipticis aristatis, arillis ovatis costatis glabris rostratis.— 6. C. *aristata*, trigyna; vaginis elongatis sulcatis, spicis cylindraceis strictis apice masculis; terminalibus omnino masculis, glumis late ellipticis aristatis, arillis ovalibus triquetris rostratis scabris. — 7. C. *chlorostachya*, trigyna; vaginis nullis, spicis fœmineis cylindraceis erectis pedunculatis; masculis solitariis, glumis ovato-lanceolatis acuminatis apice scabris, arillis ventricosis costatis apice rostratis bifurcis glumâ longioribus. — 8. C. *lenticularis*, digyna; vaginis nullis, spicis fœmineis filiformibus pedunculis patulis; masculis solitariis pedunculatis, glumis cuneatis: acumine longo spinuloso, arillis cuneato-orbiculatis papilloso-micantibus compressis marginatis. — 9. C. *alopecuroides*, trigyna; vaginis nullis, spicis fœmineis erectis cylindraceis subsessilibus; masculis solitariis, glumis ellipticis acuminatis superne scabris, arillis lanceolatis compressis lævibus apice truncatis emarginatis.

Dec. 16.—The following communications were read:

Observations on some of the terrestrial Mollusca of the West Indies, by the Rev. Lansdown Guilding, B.A. F.L.S.

The following are among the species described:

Helicina occidentalis, corpore livido, dorso tentaculisque atris,

atris, oculis prominulis.—In montibus sylvosis Sancti Vincentii. —*Bulimus hæmostomus*, corpore olivaceo-nigro corrugato: pede subtus pallido: capite bifariam crenato.—In dumetis Antillarum. —*Bulimulus stramineus.*—*Pupa undulata.*—An Account, by the same gentleman, was also read, of some rare West Indian Crabs.

The reading of Mr. John Murray's Account of his Experiments and Observations on the light and luminous Matter of the *Lampyris noctiluca*, or Glow-worm, was concluded on this evening. The writer, after detailing the opinions of various naturalists on the nature and cause of the light of the glow-worm and other luminous insects, proceeds to relate his own observations and experiments, which show that this light is not connected with the respiration, nor derived from the solar light; that it is not affected by cold, nor by magnetism, nor by submersion in water. Trials of immersion in water of various temperatures, and in oxygen, are detailed. When a glow-worm was immersed in carbonic acid gas, it died shining brilliantly: in hydrogen it continued to shine, and did not seem to suffer. Mr. Murray infers that the luminousness is independent not only of the respiration, but of the volition and vital principle. Some of the luminous matter obtained in a detached state was also subjected to various experiments, from which it appears to be a gummo-albuminous substance mixt with muriate of soda and sulphate of alumine and potash, and to be composed of spherules. The light is considered to be permanent, its occultations being caused by the interposition of an opaque medium.

The Society adjourned to January 1824.

We have to announce to our scientific readers, that the first Anniversary Meeting of the Zoological Club of the Linnæan Society of London, the establishment of which has been for some time in contemplation, was held at the Rooms of the Society on the 29th of November, the birth-day of our celebrated countryman John Ray. The Club is composed of members of the Linnæan Society devoted to the study of Zoology and comparative anatomy, and has been organized with the view of advancing the knowledge of those sciences in all their branches under the sanction of the present Society. The Club will not have any publications of their own, but will submit all original communications made to them to the Council of the Linnæan Society, to be dealt with as other communications made to the Society. The meetings of the Club, at which all the members of the Linnæan Society are entitled to be present, take place at the Society's Rooms in Soho-Square,

Square, at eight o'clock in the evening, on the second and fourth Tuesdays of every month throughout the year.

Before the Club proceeded to the election of their Officers and the other business of the day, the following opening Address, explanatory of the views of the Club, was delivered by the Rev. Wm. Kirby, who was called unanimously to the chair.

Address of the Chairman (*Rev.* William Kirby, *M.A. F.R. and L.S.*, *&c.*) *read at the Meeting of the Zoological Club of the Linnæan Society held at the Society's House in Soho Square, Nov.* 29, 1823.

Gentlemen,

Before we proceed to business, permit me to address a few words to you, upon what appear to me to be the principal objects of our association, and upon the best methods of carrying them into effect. I see many Gentlemen here present who, from their more extended knowledge of every branch of the science from which we take our name, are much more competent than myself to perform this task to your satisfaction, and upon some one of them I could wish it had devolved: but as your kindness has placed *me* in this chair, I will endeavour to fulfill this part of my official duty to the best of my abilities. I must previously state, however, that particular circumstances and engagements have unavoidably prevented my putting my thoughts together till after my arrival in town. They have, in consequence, been arranged more hastily than I could have wished, and without the aid of books. I must therefore solicit your indulgence for any imperfections of style or matter that may strike you in this address.

Zoology may be regarded as including several provinces, in every one of which our knowledge is at present very imperfect; and therefore contributions upon every subject which they include, as your taste and turn of mind may lead you, provided there is no waste of time and talent upon what is trivial and uninteresting, or has been already thoroughly investigated, will be acceptable and valuable.

There is one of these provinces that I think ought to stand high in the esteem of every *patriot* Zoologist—I mean the study of the animals that are natives or periodical visitants of his own country. An indigenous *Fauna* is the first desideratum in our science; and could a work of this kind be accomplished in every country, regard being had to natural boundaries, we might hope to become acquainted with all the prinpal groups of animals, and get a much more correct idea, than with our present imperfect knowledge we can attain

to, of the genuine *Systema Animalium*, with all its affinities and analogies as concatenated and contrasted by its Great Author.

With respect to Great Britain, in our sister science of Botany a vast deal more has been effected than in Zoology. Our indigenous Floras, if we may form a judgement from the very few new plants, that after a very general investigation of the three kingdoms have been discovered, contain nearly a complete list of its *phænogamous* vegetable productions. In the *cryptogamous* department more numerous discoveries may be expected; but still even here the Botanist is before the Zoologist, at least with regard to *invertebrate* animals. The *Vertebratæ* indeed of our islands, with the exception perhaps of those that inhabit our seas, are already, for the most part, well known and described; and all that seems to be wanted here is a more perfect acquaintance with their manners and economy, and with the varying appearances put on by some of them,—I speak particularly of the *birds*, in different periods of their growth. But undescribed British invertebrate animals daily flow in upon us in shoals; and perhaps it would not be speaking too largely were I to assert, that, excepting the *Lepidoptera* order in insects (for a more complete knowledge of which we are indebted to a gentleman near me*) not one in ten, and in some orders not one in twenty,—I speak this with regard to insects, and under the eye of a friend † who can correct me if I have made an overcharged statement,—have been described as British. What is the cause of this difference between the two sister sciences? It has happened, because perhaps the beauties with which Flora allures us, are more open to general view and require less investigation; that Botany has the advantage of first attracting the regards of the admirers of nature; and as she started first, so of course she has made the greatest progress. But Zoology is now marching after her with rapid strides, and I trust will in time overtake her, so that the sisters may run the remainder of their race, as they should do, hand in hand together. Another cause is the infinite number, even of indigenous species, of the invertebrate animals, so that it should seem that a complete Fauna, if undertaken by a single individual, must be left as a legacy to a successor for completion. *Vita brevis, Ars longa*, is a most discouraging apophthegm to the general zoologist, who without Herculean stamina undertakes the labours of a Hercules: but *Vis unita fortior*, what one man cannot hope to accomplish in the usual term of human life, may easily and well be done where many unite their forces for that purpose. Did a number of individuals, sufficiently conversant with their

* Mr. Haworth. † Mr. Stephens.

science,

science, combine to produce a British Fauna, each undertaking a separate department suited to his talents and previous pursuits, this grand desideratum might at length be effected. It strikes me that this object might be put in train by the means and under the patronage of the Zoological Club. I see now around me a number of Gentlemen sufficiently learned in nature, and several who have drunk deeply at her well-spring of knowledge, who, if once they undertook the task, would accomplish it with the highest credit to themselves and to the great advantage of the science they cultivate. Let the members of our new-born institution, amongst other subjects, discuss this point amongst themselves at their meetings—weigh the difficulties—investigate the means—consider the proper persons—apportion the work—set their shoulders to the wheel, and the thing will half be done; for most true is that aphorism—

Dimidium facti, qui bene cœpit, habet.

But let me not be misunderstood on this subject: I do not mean that such a work should be read at our meetings, or appear in the Transactions of our venerable Parent Society. This would be inconsistent with the nature of a Fauna, which ought to be published in a different form, and appeal more directly to the public for support on the ground of its own merits.

Another important object of our association with regard to indigenous Zoology is this—That insulated observations made by individuals upon the habits and economy of animals may not be lost. Few persons have an opportunity of tracing the whole proceedings and life of any species of animal; but almost every one has it in his power to relate some interesting trait, to record some illustrative anecdote, of the beings that he beholds moving around him in every direction. None of these fragments should be lost, since each may lead to important conclusions; and the whole concentrated may often form a tolerable comment, and throw great light on some perplexing text of nature. Under this head I may observe, that peculiar care and caution are requisite in noting the habitats and food of animals, particularly insects; since great mistakes have arisen, and been propagated by high authority*, from collectors being too hasty in forming their opinions on this subject.

Bare catalogues of the animals of a district, as such, are of

* For instance, *Curculio Alliariæ* L. (*Rynchites* Herbst) really feeds upon the *hawthorn*, from which it may readily be conceived to drop frequently upon *Erysimum Alliaria*, which always grows in hedges; and *Rynchænus Fragariæ* F. (*Orchestes* Oliv.) feeds upon the beech, from which it may have dropped upon the strawberry.

 little

little interest or utility; but when the localities of the *Animalia rariora* are given, or a district catalogue is worked into a *catalogue raisonné*, and includes facts before unknown with regard to the animals it registers, it becomes a useful document. To note the soil, the kind of country and atmosphere that particular animals affect, makes such a catalogue more interesting. The relative proportion, where glimpses of it can be obtained, that different species bear to each other, or their numerical distribution in any given district, is a speculation worthy of the attention of the zoologist; and likewise to obtain as full an account as possible of those which are particularly detrimental to us either in the garden, the orchard, the forest, or the field.

No papers will be more interesting than those which pursue the history of an individual through its different states; and nothing is more important for the satisfactory elucidation of natural groups of insects, and in many cases to prove the distinction of kindred species, than the knowledge of their larvæ.

The above, and many others that I might name did the time permit, appear to be legitimate objects of a Zoological Society with respect to our *indigenous* animal productions. What further observations I have to submit to your consideration will relate to Zoology in *general*. No one who wishes to be at home on the subject will confine his attention to the animals of his own country. Doing this, he will acquire only shreds and patches of knowledge, and see nothing in its real station.

When we consider the infinite number of nondescript animals, especially of insects, with which our cabinets swarm—the hosts of new forms that meet our eyes in every collection—the zoological treasures that our ships, whose sails overshadow every navigable sea, are daily bringing into our ports, we cannot help lamenting that these, for the most part, must remain ——— *sine nomine turba.*

But let us flatter ourselves that the society, whose birth we may date from this auspicious day*, will be the instrument of bringing to light and knowledge many a curious and interesting group, which would otherwise have remained unknown. *Nomina si pereunt, perit et cognitio rerum*, says Linné. Names are the foundation of knowledge; and unless they have "a name" as well as "a local habitation" with us, the zoological treasures that we so highly prize might almost as well have been left to perish in their native deserts or forests, as have

* Nov. 29, the birth-day of Ray.

grown

grown mouldy in our drawers or repositories. But when once an animal subject is named and described, it becomes a *κτημα ες αει*, a possession for ever, and the value of every individual specimen of it, even in a mercantile view, is enhanced.

It is extremely desirable, when gentlemen, moved by such considerations, set about naming and describing the animals, hitherto not so distinguished, which their cabinets contain, that they should copy the example of a learned friend near me*, who has done this in a style of superior excellence, and endeavour to elucidate natural groups; as this will, more than any other method, tend to set wide the limits of our knowledge in this department: but at any rate we ought to avoid giving insulated descriptions of a single species, unless it be remarkable either for its economy or structure; or belongs to a genus containing few known species; or fills a gap in any group. With regard to *indigenous* animals, it seems more important that new species should be described as they are discovered, this being a piece of domestic intelligence, which always comes home to us.

When we are engaged in the study of animals, and more especially of groups of them, it is of the first importance, if we would avoid mistakes, that our attention should be kept alive to what the friend lately alluded to has said on the subject of affinity and analogy. By his judicious observations on this subject he has opened a new door into the temple of nature, and taught us to explore her mystic labyrinths, guided by a safer clue than we were wont to follow. And whoever casts even a cursory glance over her three kingdoms will every where be struck by resemblances between objects that have no real relation to each other. He will see on one side *dendritic* minerals, on another *zoomorphous* plants, on a third *phytomorphous* animals; and amongst animals themselves he will see numberless instances of this simulation of affinity where the reality of it does not exist. From this part of the plan of the Creator we may gather, I think, that every thing has its *meaning* as well as its *use*; and that probably to the first pair the Creation was a book of symbols, a sacred language; of which they possessed the key, and which it was their delight to study and decypher.

But to return from this digression.—Every circumstance connected with the geographical distribution of animals is extremely interesting and important, and merits our full attention. There is often something very remarkable in the range of particular tribes and genera. Some animals, for instance, are common both to the Old World and the New, while

* Mr. W. S. MacLeay.

others

others occupy a more limited station; some have as it were their metropolis, from which as they recede, they become gradually less numerous. Some again that are found inhabiting the *plains* of a *cold* country, take their station on the *mountains* of a *warmer* one. Every quarter or principal district of the globe has likewise its peculiar types, so that a practised zoologist can often lay his finger upon an animal that he never saw before, and say confidently, This is of *Asiatic* origin—this of *African*—this of *American*—this of *Australasian:* and even in cases where creatures from these countries are apparently synonymous with those of *Europe*, there is, not unfrequently, a note of difference, that speaks their *exotic* birth. As the importance of assigning their genuine country to our animal specimens is now universally acknowledged, it would be a very useful labour, and form a very valuable communication, would any gentleman, properly qualified, undertake the correction of some of the numerous errors, with regard to their real *habitat*, that zoologists have propagated concerning the animals they have described.

I must not pass without notice another branch of our science of the deepest interest and highest importance, and more particularly as we have to lament that hitherto it has been very imperfectly cultivated, especially with regard to invertebrate animals, in these islands,—I mean *the Comparative Anatomy* of animals. France, in which this science has attained to its acme, can boast of her Cuvier, Savigny, Marcel de Serres, De Blainville, Chabrier, and others; Germany of her Blumenbach, Ramdohr, Treviranus, Herold, and a host besides; Italy of her Malpighi, Spallanzani, Scarpa, and Poli; Holland of her Swammerdam and Lyonnet: but the only boast of Britain, an illustrious one indeed, *nec pluribus impar*, in this department, is her Hunter; and even he, if my recollection does not fail me, employed his scalpel chiefly on the higher orders of animals. Medical gentlemen who cultivate this province have usually, perhaps, the human subject too much in their view, and do not always recollect, that to compare one of the lower animals with this, without making a gradual approach to it by the study of the structure of the intervening groups, must inevitably lead them to erroneous conclusions. When it is recollected that some of the most eminent comparative anatomists have not been professional men, I trust it will stimulate zoologists in general to labour in this field. I beg not to be misunderstood in what I have here stated. I have the highest possible opinion of the medical gentlemen of my country in every branch of their profession; I venerate their skill and science: but the most important

duties

duties of their station imperatively call on them to look principally at the human subject: it is not wonderful, therefore, that they should feel disposed to refer all minor forms immediately to that standard.

The zoologist has still other objects, and those of no common interest, that merit his attention. The busy world of animals that *move* around him, does not include the whole circle of his science; there are others that call to him from the *dust*, victims of that mighty catastrophe that once overwhelmed our globe and its inhabitants,—antique forms that have not yet been met with by those " that run to and fro to increase knowledge." These also, from the giant mammoth and megatherium to the most minute grain of an oolithe, afford a legitimate subject to the zoologist; and amongst our members we number some who have highly distinguished themselves in this vast arena.

To conclude. There is one other and great object which ought to stand first with every Naturalist or Association of Naturalists, the mention of which cannot with any propriety be omitted by *me*, especially upon the natal day of that illustrious Englishman, the father and founder of Natural History in this our country, whose delight it was to celebrate " the Wisdom of God in the Creation,"—that great object is the Glory of the Omnipotent Creator. "*Finis creationis telluris*," says the immortal Swede, "*est gloria Dei ex opere naturæ per hominem solum.*" We fulfill this great end when we ascribe to him the glory of his works; and more especially when, setting aside, as much as possible, every false bias, our great aim is to discover the *truth* of things, their real nature and relations. And may we all with patient assiduity walk in this path, " and proving all things, may we finally hold fast that which is good!"

The following members of the Club were appointed to form the Committee for the management of the affairs of the Club for the ensuing year:

Joseph Sabine, Esq.; Rev. Wm. Kirby; Adrian Hardy Haworth, Esq.; Nicholas Aylward Vigors, Esq.; Thomas Horsfield, M.D.; James Francis Stephens, Esq.; Mr. Thomas Bell; Mr. Edward Turner Bennet; George Milne, Esq.

And the following members were elected Officers for the same period:

Joseph Sabine, Esq., Chairman;
James Francis Stephens, Esq., Treasurer;
Nicholas Aylward Vigors, Esq., Secretary.

[We are glad to express our satisfaction at this new plan for the promotion of a branch of knowledge not at present in a due state of advancement in this country; as it will serve to produce co-operation and increased activity amongst our Zoologists,

logists, without detaching them from the general body of the cultivators of natural history, and without increasing that subdivision of them into detached and insulated Societies, which perhaps has already been carried to excess among us.—EDIT.]

HORTICULTURAL SOCIETY.

Nov. 4th. The following communications were read:

Description of a Pear-tree on which the Operation of *reverse* Grafting had been performed. By Mr. William Balfour, Gardener to the Earl Grey, at Howick, Northumberland.

Observations on the Effects of the Winter of 1822–3 on tender Exotic Plants growing in the open air at Kingsbridge, Devonshire. By Abraham Hawkins, Esq., F.H.S.

On a Method of destroying Caterpillars. By Mr. Henry Ross, Corresponding Member of the Society.

Additional Notes on the Utility of Grafting Wax. By David Powell, Esq.

Nov. 18th. The Silver Medal of the Society was presented to William Wells, Esq., F.H.S., for his attention to the improvement of Horticulture, and for his success in raising new Varieties of Double, Semi-double and Single Dahlias, Specimens of which have been shown at the Meetings of the Society.

The Silver Medal was also presented to Frederick Gar-sham Carmichael, Esq., F.H.S., for his attention and skill in Horticulture, as evinced by the Specimens of Fruits shown by him at various Meetings of the Society.

Dec. 2nd. The Silver Medal of the Society was presented to Mr. Robert Buck, Corresponding Member of the Society, Gardener to the Lord Bagot, for his skill in the Cultivation of Pine Apples, as evinced in the several Seedling Fruits shown by him at the different Meetings of the Society.

The following communications were read:

An Account of a new Variety of Plum. By the President.

On the Cultivation of the Pine Apple in low Temperature. By Mr. Archibald Stewart of Valleyfield, N.B.

GEOLOGICAL SOCIETY.

Dec. 5. A Paper was read, entitled Remarks on the Geology of Siam and Cochin-China, and certain Islands in the Indian Archipelago and Parts of the adjacent Continent. By John Crawford, Esq. M.G.S.

Dec. 19. A Paper was read containing Geological Observations collected in a Journey through Persia from Bushire in the Persian Gulf to Teheran. By James B. Fraser, Esq. M.G.S.

The author is of opinion that both the east and west sides of

of the Persian Gulf, to a great extent, consist of a calcareous formation, which, it is ascertained, in many parts continues far inland. In a part of this formation, his route from Bushire commenced; between which place and Shiraz, the hills are composed of sulphates and carbonates of lime, and the strata often much disturbed. Through a large tract of this country, carbonate of lime is intermixed with the gypsum; but in parts rocks of pure gypsum occur, and very frequently accompanied by salt. Streams and lakes of salt abound, and there is a considerable one of the latter at Shiraz. Proceeding northward, the route from Shiraz to Ispahan, a distance of about 250 miles, lies over an elevated country, the nature of which is similar to that before described, but the carbonate of lime predominates. Between the village of Gendoo and the town of Yesdikhaust Mr. Fraser found clay slate, and a conglomerate rock inclosing pebbles of quartz, greenstone and limestone cemented by carbonate of lime; strata of this aggregate rock alternate with a finer sandstone. The mountains between Ispahan and Teheran are of a character very different from the preceding; among them clay slate was observed, and the highest region, which reaches a great elevation, consists of granitic rocks.

ASTRONOMICAL SOCIETY.

Dec. 12.—Two papers were read this evening: the first being a very elaborate and able Preface written by the Foreign Secretary, J. F. W. Herschel, Esq., to accompany and explain a series of Tables for calculating the Places of the principal Fixed Stars, which have been computed by order of the Society, and will be printed in the forthcoming volume of its Memoirs;—and, 2dly, A Supplement to a former paper read before the Society on the Theory of Astronomical Instruments, by Benjamin Gompertz, Esq. F.R.S. and M.A.S.

METEOROLOGICAL SOCIETY.

At the second Meeting of this Society, held on Wednesday, Nov. 12, as mentioned in our last Number, the following gentlemen were chosen to fill the offices of President and Treasurer, and to form the Council.

President.—Geo. Birkbeck, M.D. M. Ast. Soc. M.G.S., &c.

Treasurer.—Henry Clutterbuck, M.D.

Council.—John Bostock, M.D. F.R.S.; J. F. Daniell, Esq. F.R.S; William Shearman, M.D.; Thomas Forster, M.B. F.L.S.; C. J. Roberts, M.D.; Luke Howard, Esq. F.R.S.; Richard Taylor, F.L.S.; E. W. Brayley, Jun. Esq.

A Sketch of a Code of Laws for the regulation of the Society having been read, a Committee was appointed to revise and amend the same; and it will be submitted for adoption to a General Meeting of the Society, which will be held on

 Wed-

Wednesday, January 14; and which will be resolved, when the legislative business has been concluded, into an Ordinary Meeting, for the reading of papers, &c.

XCVI. *Intelligence and Miscellaneous Articles.*

MR. POND AND M. BESSEL.

In our last Number we referred to a singular rumour, which had been circulated with much industry, relating to the *bending* of the telescope attached to the meridian circle at Königsberg: and we ventured to *contradict* that report, on the authority of the gentleman to whom the communication was said to have been first made. We have since seen letters from M. Bessel himself, of whom inquiries had been expressly made relative to this assertion; in one of which he expresses himself thus:

"With respect to my catalogue of the declinations of the principal stars, I think the information which you sent me must be founded on some misunderstanding, since I have not the least suspicion that it is wrong. The *effect* produced by the bending of the telescope of my circle, appears to me to be so well determined that, in this respect, I can expect no further improvement without running the risk of greater inaccuracies. In my method, both of observation and computation, I have never neglected any thing that could have any influence of consequence: therefore I cannot throw any light on what you say, unless some one would point out inaccuracies at present unperceived by me, which might produce an alteration.

"The whole of my proceedings are laid open to every astronomer in the 7th number of my Observations: and those who devote to them an attentive examination, will have greater confidence in what I have stated, than by listening to any idle reports."

In another letter M. Bessel expresses himself still more forcibly: but it is unnecessary to multiply this evidence, as we presume the public is by this time convinced of the falsehood of the report above alluded to.

ASTRONOMICAL INFORMATION.

The *Connaissance des Tems* for 1826 is arrived; and contains, as usual, a variety of interesting papers *which have been read at the Board of Longitude at Paris*, and which that learned body present annually to the public. The first is a communication from M. Gambart junior, director of the observatory at Marseilles, of *numerous* observations made during the years 1820, 21 and 22, of occultations, eclipses of Jupiter's satellites, eclipses of the moon, comets, &c.; and affords a

remarkable

remarkable example of the great good that may be effected by an active and intelligent observer, with even very ordinary instruments. Amongst the other papers we notice a very valuable one by M. Mathieu on some experiments made by the French, on the invariable pendulum, in the southern hemisphere; and in which will be found some new and interesting matter. These experiments are compared with those made by Sir Thomas Brisbane in New South Wales (which appear to have been communicated to the Board of Longitude at Paris, as well as to the Royal Society of London): and the result produces nearly the same compression of the earth as that previously deduced by Capt. Kater. Prior to the sailing of the expedition, M. Arago assembled Cap. Duperrey and his principal officers at the Royal Observatory at Paris, and instructed them in the mode of conducting the delicate observations which they were about to make, and of handling the various instruments that would be necessary for that purpose. In mentioning this gentleman's name, we observe with much pleasure that he has been raised, in the Board of Longitude, to the class of *Astronomes*, in the place of M. Delambre deceased: whilst MM. Nicollet and Damoiseau are the two new members added to the class of *Astronomes Adjoints*. The latter (it may be remembered) has lately made himself celebrated by his new formulæ for the lunar tables, inserted in the *Connaissance des Tems* for 1824; and for which he appears to have been rewarded with the cross of the order of *St. Louis*, and of the *Legion of Honour*. These facts show that our neighbours are alive to the advancement of astronomy, and to the promotion of the best interests of the country.

We regret that M. Schumacher's *Astronomische Hülfstafeln* for 1824 are not yet arrived.

LENGTH OF THE PENDULUM AT PARAMATTA.

In Capt. Kater's account of Sir T. Brisbane's experiments made with an invariable pendulum in New South Wales, Philosophical Transactions 1823, p. 323, he thus states the general results of them:

" If the number of vibrations resulting from Sir Thomas Brisbane's experiments at Paramatta be compared with the mean number of vibrations made by the pendulum at London, we shall have 39·07696 inches for the length of the pendulum vibrating seconds at Paramatta; ·0052704 for the diminution of gravity from the pole to the equator; and $\frac{1}{293\cdot 84}$ for the resulting compression; the length of the pendulum vibrating seconds at London being taken at 39·13929 inches.

" The experiments at Paramatta being compared with those

made by me at Unst, in latitude 60° 45′ 28″ north, give ·0053605 for the diminution of gravity from the pole to the equator, and $\frac{1}{303 \cdot 93}$ for the resulting compression.

"If Mr. Dunlop's experiments at Paramatta be compared with those made at London, we obtain 39·07751 for the length of the seconds' pendulum at Paramatta, ·0052238 for the diminution of gravity from the pole to the equator, and $\frac{1}{291 \cdot 83}$ for the compression. Or, comparing Mr. Dunlop's experiments with those made at Unst, we have ·0053292 for the diminution of gravity from the pole to the equator, and $\frac{1}{301 \cdot 09}$ for the resulting compression.

"The compressions here deduced must not as yet be deemed conclusive; for it is well known that a very small alteration in the number of vibrations made by the pendulum would occasion a considerabie difference in the fraction indicating the compression. The indefatigable zeal of Sir Thomas Brisbane will, however, no doubt soon furnish additional data."

PRESERVATION OF GREENHOUSE-PLANTS.

It has been ascertained, by Mrs. Tredgold, that plants may be completely protected from the depredations of insects by washing them with a solution of bitter aloes, and the use of this wash does not appear to affect the health of the plants in the slightest degree. And wherever the solution has been used, insects have not been observed to attack the plants again. As there is much difficulty in preserving a small collection by the usual methods, this notice of a simple remedy may be very useful.

ROMAN CEMENT.

According to an analysis lately made by M. Berthier, the component parts of Parker's cement are:

Carbonate of lime	·657
——— magnesia	·005
——— iron	·070
——— manganese	·019
Clay silica	·180
—— alumina	·066
Water	·013
	1·000

M. Berthier is of opinion, that with one part of common clay and two parts and a half of chalk, a very good hydraulic lime may be made, which will set as speedily as Parker's cement. He concludes from many experiments, that a limestone containing six per cent. of clay affords a mortar perceptibly hydraulic. Lime containing from 15 to 20 per cent. is very hydraulic; and when from 25 to 30 it sets almost instantly, and may therefore be held to be, to all intents and purposes, real Roman cement.

SIR

SIR WALTER SCOTT ON OIL-GAS.

At a late Meeting of the Edinburgh Oil-Gas Company, Sir Walter Scott said, that he had now had three months experience of Oil Gas light in his house at Abbottsford, and he could assure the Meeting that nothing could be more pleasant, more useful, safe, and economical. He was sure the expense was not the twentieth part of what it had formerly cost him for oil and candles. The light itself was greatly superior, was extremely cleanly, saved much trouble to servants, and did not produce the least smell, or the least injury. Not only could it be used in kitchens and dining-rooms, but it was extremely useful in bed-rooms, where a flame could be kept up during the whole night so minute as to be scarcely perceptible, which could be enlarged to a powerful light in an instant at any hour when wanted. It was also very safe; at least it was much safer than common lights, for it was not carried from place to place as common lights were, and unless combustibles were brought to it no danger could arise. The light was indeed so convenient, cheap, and delightful, that were it once introduced, he was convinced it would be used within two years in every private house in Edinburgh.—*Scotsman, Nov.* 29, 1823.

EXPANSION OF GASEOUS FLUIDS.

According to the experiments of Gay-Lussac, which have been verified by Dulong and Petit, the expansion of air and other gaseous fluids is nearly $\frac{1}{480}$ part of the bulk for one degree of heat, measured by Fahrenheit's scale, when the temperature is not increased beyond 212°. But, according to Dulong and Petit, the expansion is less in high temperatures. Taking the expansion for the whole range from the freezing point of water to the boiling point of mercury, the expansion for each degree would be only $\frac{1}{493}$, supposing it to be equable.

If we consider the expansion to be equable, and make A the bulk of the gas at the inferior temperature, and B its bulk when its heat is increased t degrees, and $\frac{1}{\varepsilon}$ the expansion for one degree, we have

$$A\left(1+\frac{t}{\varepsilon}\right)=B.$$

Or, $\frac{A(\varepsilon+t)}{\varepsilon}=B$, when the temperature is increased; and

$\frac{B\varepsilon}{\varepsilon+t}=A$, when the temperature is diminished.

If $\varepsilon=480$, the formulæ are

$\frac{A(480+t)}{480}=B$, when the temperature is increased; and

$\frac{480\,B}{480+t}=A$, when the temperature is diminished.

For

For example: Let the temperature of 100 cubic inches of gas be 32°, and it is required to find the bulk at 212°; then $t=212-32=180$, and $A=100$, hence $\frac{100(480+180)}{480}=137{\cdot}5=B$.

Again: Let the bulk at 212° be 137·5 cubic inches, required the bulk at 32°. In this case also we have $t=180$, and $B=137{\cdot}5$, hence

$$\frac{480\times 137{\cdot}5}{480+180}=100=A.$$

Another example may be taken when the temperature of 100 cubic inches is 50° to find the bulk at 60°; in this case $t=10°$, and $A=100$, therefore

$$\frac{100(480+10)}{480}=102{\cdot}083\dot{3}.$$

These will be sufficient to show that the gentleman who has attempted to correct the writers on chemistry, has given a rule which is not perfectly accurate (see Phillips's Annals of Philosophy for December 1823, p. 415). He makes the bulk, as increased by expansion, in the last example only 102·008. In fact, the rules given by the chemical writers he has quoted, are accurate when the temperature is increased; while his own is only correct when the one of the temperatures happens to coincide with the freezing point. ——X.

EARTHQUAKE IN CANADA.

Quebec, Sept. 10.

On the 28th of last month, about three o'clock in the afternoon, the inhabitants of the village of Hayotte, in the parish of Champlain, were alarmed by the following extraordinary occurrence:—A tract of land, containing a superficies of 207 arpents, was suddenly moved five or six arpents (about 360 yards) from the water's edge, and precipitated into the river Champlain, overwhelming in its progress barns, houses, trees, and whatever else lay in its course. The earth thus removed dammed up the river for a distance of 26 arpents. The effect was instantaneous, and accompanied by an appalling sound; a dense vapour, as of pitch and sulphur, filled the atmosphere, oppressing those who witnessed this awful convulsion almost to suffocation. The course of the river being thus obstructed, the waters swelled to a great height, but must rise seven or eight feet more before they can find a passage. Various causes are at present assigned for this singular phænomenon—such as the effect of a volcanic eruption, or an earthquake; and by others it is supposed to have been produced by the water having insinuated itself between the strata of clay and the subjacent bed of sand.

LIST

LIST OF NEW PATENTS.

To Joseph Bourne, of Denby, Derbyshire, stone-bottle manufacturer, for certain improvements in the burning of stone- and brown-ware in kilns or ovens, by carrying up the heat and flame from the furnace or fire below to the middle and upper parts of the kiln or oven, either by means of flues or chimneys in the sides thereof, or by moveable pipes or conductors to be placed within such kilns or ovens; and also by increasing the heat in kilns or ovens by the construction of additional furnaces or fires at the sides thereof, and to communicate with the centre or upper parts of such kilns or ovens; also by conveying the flame and heat of one kiln more into another or others by means of chimneys or flues, and thus permitting the draft and smoke of several kilns or ovens to escape through the chimneys of a central kiln or oven of great elevation, whereby the degree of heat is increased in the several kilns or ovens, and the quantity of smoke diminished. —Dated 22d of November 1823.—2 months allowed to enrol specification.

To John Slater, of Saddleworth, Yorkshire, clothier, for certain improvements in the machinery or apparatus to facilitate or improve the operation of cutting or grinding wool or cotton from off the surfaces of woollen cloths, kerseymeres, cotton cloths, or mixtures of the said substances, and for taking or removing hair or fur from skins.—22d Nov.—2 mo.

To Thomas Todd, of Swansea, South Wales, organ-builder, for his improvement in producing tone upon musical instruments of various descriptions.—22d November.—6 months.

To Samuel Brown, of Windmill-street, Lambeth, Surry, gentleman, for his engine or instrument for effecting a vacuum, and thus producing powers by which water may be raised and machinery put in motion.—4th December.—6 months.

To Archibald Buchanan, of Catrine Cotton Works, one of the partners of the house of James Finlay and Co., merchants in Glasgow, for a certain improvement in machinery heretofore employed in spinning-mills in the carding of cotton and other wool, whereby the top cards are regularly stripped and kept clean by the operation of the machinery without the agency of hard labour.—4th December.—4 months.

To Josiah Parkes, of Manchester, Lancashire, civil engineer, for a certain method of manufacturing salt.—4th December.—6 months.

To George Minshaw Glascott, of Great Garden-street, Whitechapel, Middlesex, brass-founder, and Tobias Michell, of Upper Thames-street, London, gentleman, for their improvements in the construction or form of nails to be used in or for the securing of copper and other sheathing on ships, and for other purposes.—9th December.—6 months.

To Thomas Horne the younger, of Birmingham, Warwickshire, brass-founder, for certain improvements in the manufacture of rack pulleys in brass or other metals.—9th December.—6 months.

To William Furnival, of Droitwich, salt-manufacturer, and Alexander Smith, of Glasgow, master mariner, for their improved boiler for steam-engines and other purposes.—9th December.—6 months.

To Sir Henry Heathcote, of No. 23, Surry-street, Strand, Middlesex, knight, and captain in the Royal Navy, for his improvement of the stay-sails generally in use for the purpose of intercepting wind between the square sails of ships and other square-rigged vessels.—13th December.—6 months.

To Jarvis Boot, of Nottingham, in the county of Nottingham, lace manufacturer, for his improved apparatus to be used in the process of singeing lace and for other purposes.—13th December.—6 months.

To Pierre Jean Baptist Victor Gosset, of Queen-street, Haymarket, Middlesex, merchant, who, in consequence of a communication made to him by a certain foreigner residing abroad, is in possession of an invention of a combination of machinery for producing various shapes, patterns, and sizes from metals or other materials capable of receiving an oval, round, or other form.—18th December.—2 months.

A METEOROLOGICAL TABLE: *comprising the Observations of Dr.* BURNEY *at Gosport, Mr.* CARY *in London, and Mr.* VEALL *at Boston.*

Gosport, at half-past Eight o'Clock, A.M.								Clouds.							Height of Barometer, in Inches, &c.		Thermometer.				Rain.		Weather.	
Days of Month, 1823.	Barom. in Inches,&c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Cirrus.	Circocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Lond. 1 P.M.	Bost. $8\frac{1}{2}$ A.M.	London. 8 A.M.	London. Noon.	London. 11 P.M.	Boston. $8\frac{1}{2}$ A.M.	London.	Boston.	London.	Boston.
Nov. 26	30·26	47	52	70	NW.	...	...	...	...	1	...	...	...	...	30·25	29·94	47	49	47	47	...	...	Cloudy	Cloudy
27	30·18	46	...	70	E.	...	0·010	...	...	1	...	...	...	1	30·17	29·94	46	48	46	46·5	...	...	Cloudy	Cloudy
28	30·03	47	...	73	S.	0·05	.230	...	...	1	...	...	...	1	29·98	29·72	46	49	47	44	...	...	Showery	Rain
29	29·70	49	...	78	S.	...	·170	...	...	1	...	...	...	1	29·65	29·45	47	52	50	48	0·25	·51	Rain	Rain
30	29·50	51	...	81	SW.	·07	·640	...	...	1	...	...	...	1	29·50	29·15	52	55	57	48	...	·14	Fair	Cloudy, rain *p.m.*
Dec. 1	29·67	51	52	75	SW.	...	...	1	1	1	...	...	...	...	29·72	29·30	52	54	44	49	...	...	Showery	Stormy
● 2	29·56	51	...	83	S.	...	·315	1	...	1	...	1	...	1	29·56	29·30	47	52	50	46	...	...	Showery	Cloudy, rain *a.m.*
3	29·60	45	...	69	SW.	...	·400	1	1	1	...	1	...	1	20·65	29·25	39	49	51	39·5	...	·11	Fair	Fine, rain at night
4	29·40	45	...	66	W.	·10	...	1	1	1	...	...	...	...	29·47	28·92	45	47	40	43	...	.13	Fair	Stormy, wind W.
5	29·75	35	...	74	NW.	...	·455	1	1	1	...	...	...	1	29·83	29·55	35	44	40	38	...	·06	Rain	Fine [rain at night
6	29·43	41	...	70	NW.	...	...	...	...	1	...	...	1	...	29·75	29·45	38	42	37	40	1·10	·12	Fair	Rain
7	30·42	32	...	73	NW.	·08	...	...	...	...	...	...	...	...	30·55	30·30	32	40	35	31	...	·3	Cloudy	Fine
8	30·47	41	$51\frac{1}{2}$	78	W.	...	...	1	1	1	...	...	...	...	30·40	30·22	36	45	42	37	...	...	Fair	Fine
9	30·34	42	...	80	NW.	...	...	1	...	1	...	1	...	...	30·40	30·17	42	42	32	35·5	...	...	Fair	Fine
☽ 10	30·40	32	...	75	NW.	·05	...	1	1	1	...	1	...	...	30·38	30·22	30	40	42	32·5	...	...	Cloudy	Fine
11	30·18	45	...	75	W.	...	·005	1	1	1	...	...	...	1	30·12	29·85	42	47	47	43	...	...	Fair	Cloudy
12	29·90	45	...	72	W.	...	·015	...	...	1	...	1	...	1	29·80	29·55	47	45	35	39	...	...	Fair	Fine
13	29·95	35	...	65	NW.	·05	...	...	...	...	...	...	...	...	29·99	29·70	34	42	32	35	0·05	...	Fair	Fine
14	30·22	28	...	68	NW.	...	·005	1	1	1	...	...	...	1	30·22	30·06	30	39	40	28·5	...	...	Fair	Fine
15	30·20	39	51	73	NW.	...	·050	1	1	1	...	...	...	1	30·25	30	39	44	35	35	...	...	Cloudy	Fine
16	30·21	44	...	80	SW.	...	·200	...	...	...	...	...	1	1	30·12	30	42	47	46	37·5	...	...	Stormy	Cloudy
○ 17	29·71	46	...	78	S.	·10	·500	...	...	...	...	...	...	1	29·37	29·50	45	48	43	37·5	...	·68	Fair	Fine, rain *a.* & *p.m.*
18	29·44	36	...	73	W.	...	·330	...	1	1	...	1	1	1	29·45	29·22	35	42	35	33	...	...	Fair	Fine
19	29·54	34	...	74	N.	...	·235	...	1	1	...	...	1	1	29·70	29·50	32	34	31	27	...	...	Stormy	Fine
20	29·15	42	...	87	S.	·05	·130	...	1	1	...	1	1	1	29·19	29·27	37	44	38	33	0·30	·15	Fair	Snow, rain *a.m.*
21	29·20	38	$50\frac{3}{4}$	80	NW.	...	·010	1	1	1	...	...	1	1	29·28	29·60	34	42	40	32	...	·02	Fair	Fine, rain at night
22	29·53	37	...	79	NW.	...	·030	...	...	1	...	...	...	1	29·72	29·45	40	43	37	33·5	...	...	Rain	Cloudy
23	29·75	41	...	84	S.	·04	·220	...	...	1	...	...	...	1	29·75	29·60	37	43	50	37	...	...	Rain	Rain
☾ 24	29·92	47	...	84	W.	...	·075	...	...	1	...	...	...	1	29·95	29·75	47	47	50	39	...	·10	Cloudy	Rain
25	29.98	48	$50\frac{1}{2}$	92	SW.	·04	·192	...	...	1	...	...	...	1	29·95	29·75	46	50	47	45	...	·09	Fair	Cloudy
Averages:	29·853	42·00	51·29	75·9		0·63	4·217	12	13	26		7	6	21	29·57	29·65				38·7		2·04		

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ERRATA:

Page 153, line 8: *for* "and it may constitute the basis" *read* "and to the probability that it may constitute the basis" &c.

Page 202, six lines from bottom, the words *Dicotyledonous* and *Monocotyledonous* should be transposed.

Page 267, line 20: *for* "slips" *read* "falls." In Mr. Wright's paper on Fulminating Mercury, p. 203 &c. *for* "Chlorine of potash," *read* passim "Chlorate."

Page 360, line 5: *for* "lime" *read* "magnesia."

Page 283, first note, line 2: *for* "300°," *read* "572° F."

Page 387, line 9: *for* "the Rev. E. Jenner," *read* "Mr. G. H. C. Jenner."

Page 396, line 9 from bottom, *for* "a mixture," *read* "the mixture."

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